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## Notices of the Aëronautical Society of Great Britain

### OFFICIAL NOTICES

**May Elections:** Members:—G. H. CHALLENGER and T. S. DUNCAN.
Associate Member:—COL. H. G. FITTON.

**June Elections:** Members:—MAJOR-GENERAL S. B. VON DONOP and E. TEMPLE ROBINS.
Associate Member:—C. G. GRUNHOLD.

**Associate Fellows Election:** As a result of the ballot for Associate Fellowship, the following have been duly elected:—R. O. BOSWALL; G. H. CHALLENGER; E. H. CLIFT; H. COANDA; L. COATALEN; S. F. CODY; LIEUT. R. GREGORY, R.N.; E. C. GORDON ENGLAND; E. H. HANKIN, M.A.; BRIG.-GEN. D. HENDERSON; J. L. HODGSON; CAPT. C. H. LEY; LIEUT. C. J. L'ESTRANGE MALONE, R.N.; J. H. NEAL; J. J. NORTH; CAPT. GODFREY PAINE, R.N.; R. K. PIERSON; LIEUT. C. J. PORTE; COMM. C. H. SAMSON, R.N.; DR. T. E. STANTON; MAJOR F. H. SYKES.

**Wilbur Wright Memorial Lecture.** A Report of the first Wilbur Wright Memorial Lecture appears in this issue. The dinner to the lecturer was held in the King's Room of the Whitehall Rooms, Hotel Metropole, at seven o'clock on the evening of the lecture, and was most successful. The Society is much indebted to the Sub-Committee (Major General R. M. Ruck, C.B.; Mr. Griffith Brewer; and Mr. A. E. Berriman), who undertook the arrangements. As a souvenir of the occasion a signed portrait of Wilbur Wright and a copy of the Society's history were distributed.

The official guests were as under:—Horace Darwin, F.R.S.; The Hon. Mrs. Horace Darwin; Miss Ruth Darwin; Col. G. O. Squier, Military Attaché, American Embassy; Dr. R. T. Glazebrook, Director, National Physical Laboratory; Major-Gen. S. B. von Donop, Master-General of the Ordnance; Roger W. Wallace, K.C., Vice-Chairman, Royal Aero Club; Harold E. Perrin, Secretary, Royal Aero Club; P. J. Hannon, Secretary, Navy League; Sir J. C. E. Shelley, President, Kite and Model Aeroplane Association; The Hon. Lady Shelley; B. G. Cooper, Secretary, Aeronautical Society; Lieut. Calderara, Italian Navy; W. R. Comings; Mrs. W. R. Comings, Society of American Women in London; The Rt. Hon. the Lord Blyth.

## WILBUR WRIGHT MEMORIAL FUND

Credit :—

| | £ | s. | d. |
|---|---|---|---|
| Amount previously acknowledged .. .. .. .. | 557 | 3 | 1 |
| B. Woodward .. .. .. .. .. .. .. | 1 | 1 | 0 |
| Prof. T. A. Hearson .. .. .. .. .. .. | 1 | 1 | 0 |
| Interest on £500 on deposit to Christmas, 1912 .. .. | 4 | 14 | 10 |
| C. E. Hearson .. .. .. .. .. .. .. | 1 | 1 | 0 |
| Horace Darwin .. .. .. .. .. .. .. | 5 | 0 | 0 |
| Interest on £500 on deposit to Midsummer .. .. | 7 | 14 | 10 |
| Interest on Aero-Educational Premiums Fund .. .. | 4 | 14 | 1 |
| | £582 | 9 | 10 |

Debit :—

| | £ | s. | d. | | | |
|---|---|---|---|---|---|---|
| Cheque book .. .. .. .. .. | 0 | 4 | 0 | | | |
| Premium to Horace Darwin, F.R.S., for first Wilbur Wright Lecture .. .. .. | 21 | 0 | 0 | | | |
| | | | | 21 | 4 | 0 |
| | | | | £561 | 5 | 10 |

**National Aerial Defence Association :** On the initiative of the Aerial Defence Committee of the Navy League, a public Meeting was held at the Mansion House on Monday, 5th May, 1913, at which resolutions were passed insisting upon the necessity for Great Britain taking steps to achieve complete security against attack in the air, and providing for the formation of a "National Aeronautical Defence Association." The Rt. Hon. the Lord Mayor of London presided, and the meeting was most successful, the influential support accorded being of a most representative character. The Association is to be managed by an Executive Committee, upon which the Aëronautical Society has three representatives (Col. H. E. Rawson, C.B., R.E., Dr. R. Mullineux Walmsley, and Mr. B. Woodward). It is intended that an appeal shall be made to the public for contributions and donations to a National Aeronautical Defence Fund to be applied for the purposes of the Association. The administration of this Fund is to be controlled by not less than three Trustees, nominated by the Lord Mayor of London, and vacancies amongst whom will be filled by the Lord Mayor. County and Borough branches of the Association are to be formed.

The organisation of this Association marks a very important step forward in the organisation of Aeronautics in this country. From the point of view of this Society the most important of the objects of the Association is "(c) To assist by every means possible the extension of experiment and research in relation to the improvement of airships and aeroplanes intended for purposes of war."

**Library :** The Council desires to thank Mr. T. O'B. Hubbard for the gift of the following books :—*Les Ballons dirigeables*, E. Girard et A. de Rouville ; *Les Aérostats dirigeables*, B. de Grilleau ; *Les Dirigeables*, M. H. André ; *Aérostat dirigeable à volonté*. M. le Baron Scott ; *Vocabulaire de l'Aviateur Constructeur ; Les Ballons dirigeables*, G. Tissandier ; *Le Problème de la direction des Ballons*, R. Soreau ; and Mr. Meryon White Winton for a copy of *The Clipper of the Clouds* (reprint).

## GENERAL MEETING

THE SIXTH MEETING of the Forty-Eighth Session of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 12th February, 1913, at 8.30 p.m. Mr. Mervyn O'Gorman, M.I.M.E., A.F.Aë.S., presided.

THE CHAIRMAN, in introducing the Lecturer, said he was looking forward to what Mr. Bairstow had to say as to the Laws of Similitude. This, he was sure, would be brief and to the point, and would not perhaps indicate the large amount of hard work which had had to be put in, in order to reach the position to which they had brought the subject at the National Physical Laboratory. He hoped that in the discussion, however, some members would be able to draw upon Mr. Bairstow's very large stock of knowledge, which he had to back up his statements.

## THE LAWS OF SIMILITUDE

BY LEONARD BAIRSTOW, A.R.C.SC.

The title of this paper does not immediately suggest aeronautics, but the connection is very intimate, as the laws of similitude constitute the theory of the use of models. Aeronautics, or rather, aerodynamics, is almost entirely an experimental science at present, and a glance backwards through time will show how much of the fundamental data of the subject is derived from experiments on models. We have, in the first place, Langley's researches, which showed that sufficient lift could be obtained to make models fly with such light engines as he was able to construct. The development of the petrol motor removed the greater part of the engine difficulty, and with that out of the way, Langley's figures for model planes, modified by Lilienthal's experiments on cambered models, provided the initial information from which the earliest flying machines were constructed.

It is true that the balancing of aeroplanes, as introduced by the Wright Brothers, was a revolutionary achievement carried out in the first instance on a full scale glider, but the later considerations of inherent stability were first enunciated by Mr. Lanchester from experiments on models, whilst the data underlying Prof. Bryan's mathematical investigations of stability are obtained solely from experiments on models.

All constructors of aeroplanes are familiar with the model researches of Eiffel and use his results in their drawing office practice. They are also aware that full scale experiments are very difficult to carry out. At the present time the recorded full scale measurements are extremely few in number and are confined entirely to determinations of thrust and gliding angle. They are not nearly advanced enough to be used for a determination of the best form of aerofoil or strut. This being the position, it is evident that we must still continue to make experiments with models and in doing so must ask ourselves whether the flow round models is like that round the flying machine and if not, we must know how to find our conversion factors. A true theory of aerodynamics would answer these questions for us completely, but unfortunately for us the answers to such questions are beyond the reach of our present mathematical knowledge.

Fig. 1 illustrates a motion which has defied the mathematician. The object in the centre of the dark "bar" across the right of the picture is an aerofoil immersed in running water, and looked at from one end. The streaks are the tracks of brightly illuminated oil drops of the same specific gravity as water. The exposure was 1 second, and during that time each drop had been moving. The length of the trail shows the average velocity. *Here* the fluid is still, whilst *there* it is moving very rapidly. Over the back the pressure of eddies is clearly shown. A satisfactory theory of hydrodynamics would be able to predict all this for us; as we have not got such a theory the results have to be obtained experimentally by some such method as that by which the figure was produced. The water channel is shown in Fig. 2.

Although we cannot predict the motion in any given case we *can* have a theory which tells us how to get the same results from two models and that even when we cannot see the motion. This is the principle of similitude. The laws are not always simple, and there are an infinite number of them, only one of which is applicable to a given experiment. I hope to be able to show how we decide which law will be appropriate to the various motions with which aeronautics is concerned.

In the allied subject of ship-propulsion, as we all know, the testing of models of ships has been carried on for years, and the law of similitude is there embodied in the statement that models should be tested at a speed which is related to the speed of the ship in proportion to the square root of the length of the ship expressed as a multiple of the length of the model.

This law is known as Froude's law of corresponding speeds. On investigation it is found that this condition is necessary in order to make the waves of the same shape to scale for both the model and ship. As there are no surface waves in air, it will not be surprising that the same law is not applicable to experiments on the lift and drift of planes. On the

FIG. 1.—OIL DROPS ROUND AEROFOIL. TIME EXPOSURE (1 SECOND)

other hand it is applicable to some of the problems incidental to the study of the stability of aeroplanes, but obviously not because of the production of surface waves.

The theory involved in the laws of similitude is strictly accurate, and is just as true as Newton's Laws of Motion. It is in fact an exceedingly direct application of those laws to experiment. The data to which the theory applies are experimental and the accuracy of the results is therefore dependent on the truth of the experimental conclusions.

A simple illustration will show the whole process of reasoning involved from the beginning of an experiment to the development of the law of similitude corresponding to it. Suppose, for instance, that we have been making experiments on a simple pendulum and that as a result we have found that any alteration in the length produces a very appreciable influence on the time of oscillation. We have also found that the forces producing the oscillation are due to the attraction of the earth on the pendulum bob. If the experiment is made in air there are also small effects due to air resistance, but when the experiment is carried out in a vacuum, as it often is, nothing else appears to affect the motion.

The experimental evidence is now complete and the following theoretical problem can be investigated.

"The time of swing of a simple pendulum depends on its length, $l$, on the value of g at the place, and on nothing else; Find the law connecting the time of oscillation with the length and attraction."

The complete solution to the problem by means of a differential equation is well known to all engineers in the form $T = 2\pi\sqrt{\frac{l}{g}}$

Except that we cannot say that the constant outside the root sign is $2\pi$ we can obtain this answer without reference to differential equations. To develop this method we turn to Newton's laws and motion and from them we discover that all measurements connected with motion can be expressed in terms of the units of mass, length and time, this being usually referred to as the theory of dimensions. In the above equation we have

$$T = \sqrt{\frac{L}{\frac{L}{T^2}}} = T$$

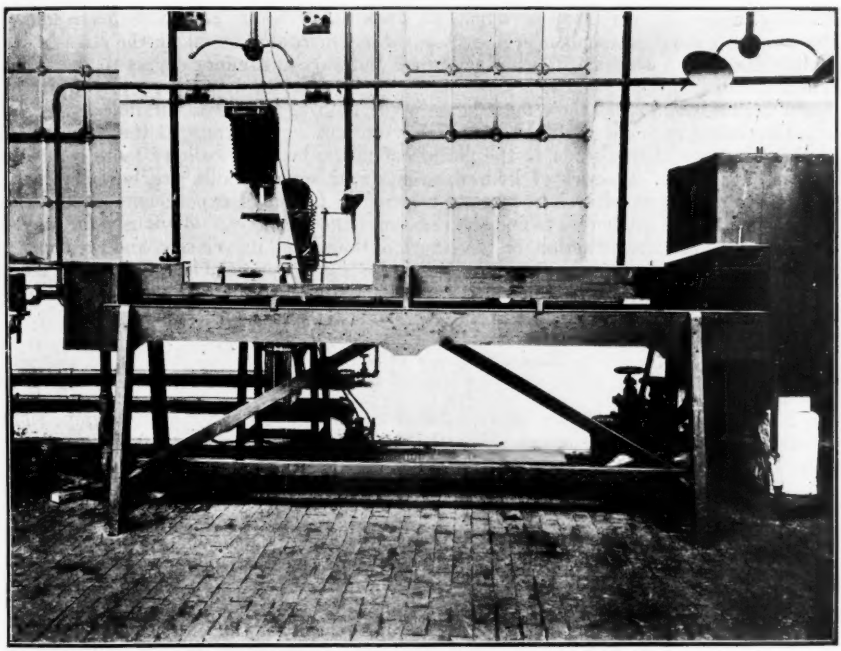


FIG. 2.—WATER CHANNEL USED FOR OBTAINING
FIGS. 3 AND 4.—(AIRSHIP MODEL CAN BE SEEN IN POSITION UNDER CAMERA)

and the dimensions on both sides of the equation agree. In fact any failure to obtain agreement is a certain sign of error in the calculation. The important point to notice is, that it is *impossible* to find any other combination of the two quantities $l$ and g which will have the dimensions of T. Hence we can say that because the dimensions agree we must have

$$T = \text{constant} \times \sqrt{l/g}$$

The process for finding the laws of similitude is simply that of finding the only arrangement of all the quantities which matter so that the dimensions on both sides of an equation agree.

If we apply the method to dirigibles and ask ourselves "if several dirigibles are made to the same drawings but to different scales, which is most likely to break, the smallest or the largest?" then, since the design is fixed, the stress depends only on the size $l$, and on the weight of the materials of which it is made w lbs. per cubic foot and we write.

$$\text{Stress} = f(w, l)$$

It is required to combine $l$ and w so that the answer has the dimensions of stress. The only possible answer is

$$\text{Stress} = \text{constant} \times wl.$$

If the materials are the same from one size to another, w is constant and we see that the bigger the dirigible the bigger the stress and that doubling the size of an airship without altering the drawings or material reduces its factor of safety to half.

The application to similar aeroplanes which are to travel at the same speed is equally easy and we find that increase of size does not produce difficulties due to increased stresses.

These rules are not new; engineers have acted on similar rules for a long time. Railway bridges of short span in which the stresses are mainly due to the train follow the aeroplane law. Long span bridges, like the Forth Bridge, in which the stresses are mainly due to the weight of the bridge girders and only in a minor degree to the train, follow the dirigible law, and it is recognised that there is an upper limit to the size of cantilever bridges, just as there must be an upper limit to the size of dirigibles.

Interesting as these applications are, they are not so important as the application of the principles of similitude to the motion of fluids, because we have more complete theories available. Theories of hydrodynamics and aerodynamics are, however, very incomplete and in naval architecture are rejected in favour of experiments on models. The particular problem here is to find the resistance due to waves. Obviously the waves depend on the earth's attraction, on the length of the model, its velocity, and the density of the fluid, and so far as is known, on nothing else to any appreciable extent. The most general expression for resistance which contains all these quantities and has the right dimensions is

$$\text{Resistance} = \rho\, v^2\, l^2\, f\left(\frac{v^2}{lg}\right)$$

Now $f$ is a general unknown function and at first sight it might appear to be impossible to make any use of the equation. A little analysis, however, shows how to avoid the difficulty if only we are in a position to test a model of the ship. On the earth $g$ is constant and if we make $v^2/l$ have the same value for the model and ship, then $f\left(\frac{v^2}{lg}\right)$ has the same value; this is just the statement of Froude's law of corresponding speeds. Now go further; we have used g as being the earth value for both model and ship. Suppose that the ship was for use on Mars and the model test was to be made on the earth, then the equation tells us that since the attraction on Mars is less than that on the earth the model must be towed faster in a given proportion. The reason for this extension of Froude's law is easy to see; since the earth attracts the water more strongly than Mars, it requires a greater disturbing force and therefore greater speed on the earth to create waves as big as those on Mars at the lower speed.

It has been known for a long time that frictional resistances do not follow Froude's law, and these forces are calculated separately. Suppose, however, that we are deeply submerged, then the surface does not matter and we are left entirely with friction. What is the law in this case?

Experimental evidence now says, the resistance depends on the speed, the length of the model, the velocity and on the density and viscosity of the fluid. As before we find

$$\text{Resistance} = \rho\, v^2\, l^2\, f\left(\frac{vl}{\nu}\right)$$

is the only possible relationship between the above quantities which has the correct

dimensions. $\nu$ is the mathematical coefficient of viscosity. The models will now produce similar disturbances in the fluid if $\frac{vl}{\nu}$ is constant. This might be called the law of corresponding speeds for fluid friction and was first stated by Osborne Reynolds. It differs from Froude's law of corresponding speeds in one particularly important respect. If we take two models of different sizes, the friction law says that the velocity past the bigger of the two must be the smaller, whilst Froude's law says exactly the opposite

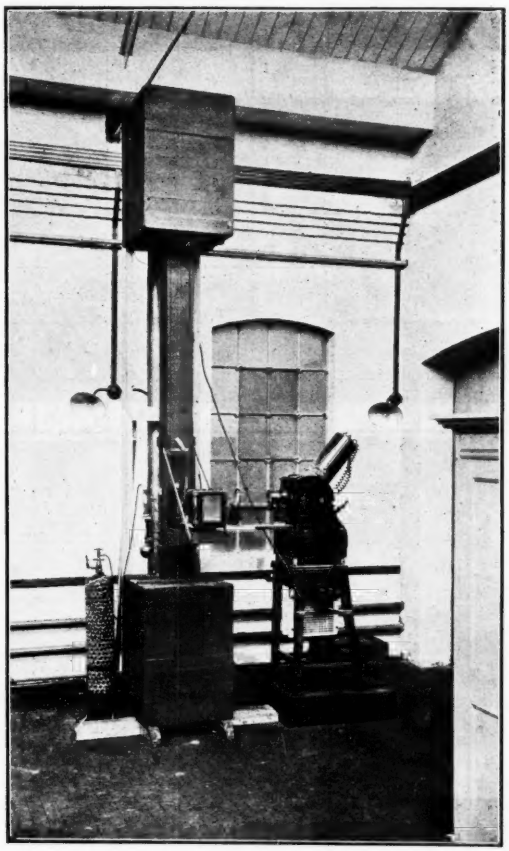


FIG. 5.—AIR CHANNEL USED FOR OBTAINING FIGS. 6 AND 7. (SMALL FLAT PLATE CAN BE SEEN IN POSITION, PARTLY BEHIND ONE OF THE SUPPORTS OF CAMERA TABLE)

Turning now to an experiment in which the earth's attraction, and therefore, presumably, Froude's law doesn't come in, we can see (Fig. 3) in a striking way how the mathematical conclusions are borne out in fact. The motion of the water at the back of the square plate inclined to a current of water is rendered visible by coating the plate with Nestlé's milk. The photograph shows a continuous corkscrew sheath in the wake of the plate. Imagine the speed to be gradually raised.

For some time nothing remarkable happens, but eventually the flow changes its character to that represented in Fig. 4. Instead of a continuous spiral streak, the eddies now come off in definite loops and there is no resemblance between the new and the old

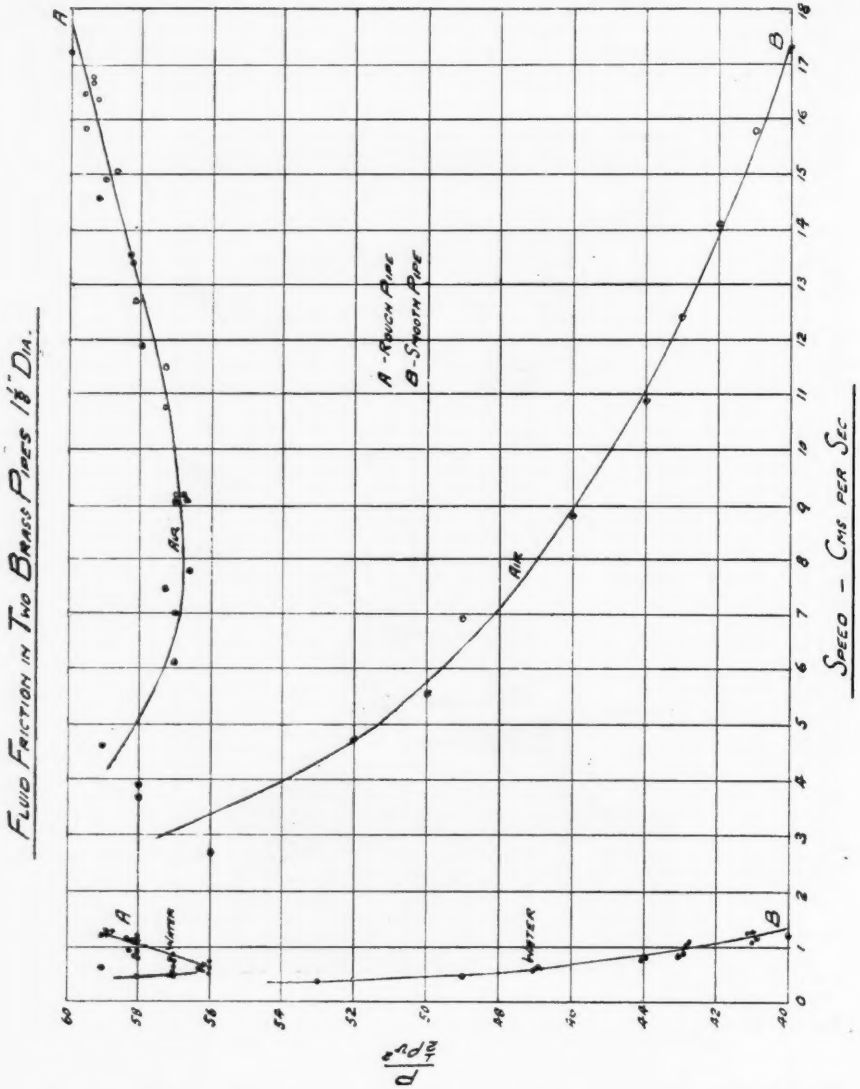



FIG. 8.—EXPERIMENTAL RESULTS ON SKIN FRICTION FOR WATER AND AIR PLOTTED WITHOUT REFERENCE TO THE FRICTION LAW OF CORRESPONDING SPEEDS

flows. Now, further, imagine the small plate removed and one twice its size substituted and the experiment again repeated to find the velocity at which the flow changes. This has been done at the National Physical Laboratory and it is shown that as nearly as it

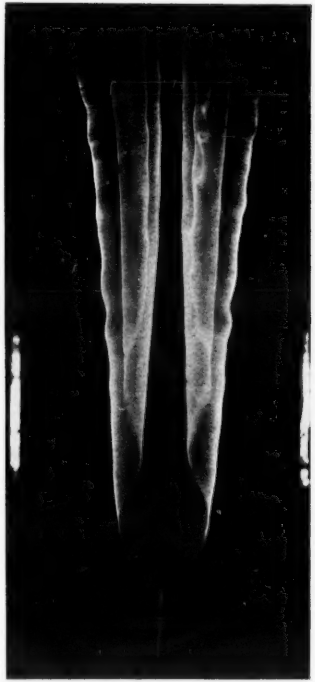


Fig. 3.

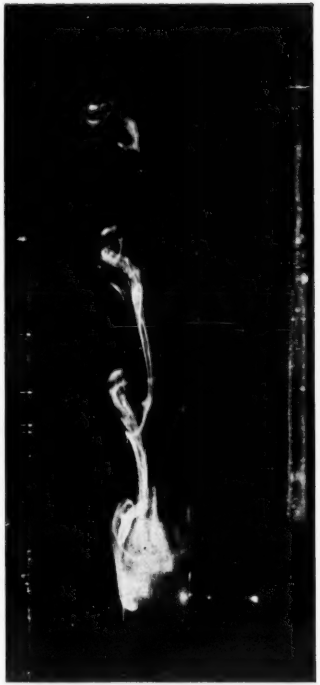


Fig. 4.

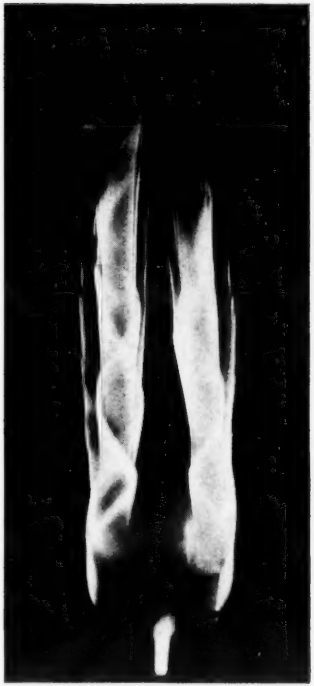


Fig. 6.

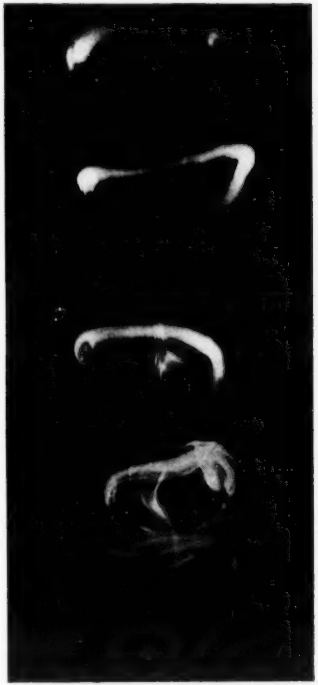


Fig. 7.

can be measured the change occurs in accordance with the friction law, *i.e.*, doubling the size of plate and halving the velocity always produces the same type of flow. This observation definitely proves that these eddies are produced by friction and do not obey Froude's law as they are often supposed to do. Now carry the experiment further and change the fluid from water to air. A channel (Fig. 5) was made to take a model twice as big as that in the water channel and the flow was made visible by smoke. Exactly the same changes were observed (Figs. 6 and 7) but the speed at which the flow changed was very much higher in the air than in the water. The mathematics say between 6 and 7 times. We have not yet been able to do the experiment so accurately as to get a better number than that deduced from the laws of similitude.

I will conclude by referring to the two other experimental illustrations of the friction law of corresponding speeds. One refers to skin friction and has a bearing on the calculation of the resistance of dirigibles and aeroplane bodies, and the other refers to the resistance of stay wires. In each case the same results have been plotted in two ways, one of which compares the observations at the same speed and the other at corresponding speeds. The simplicity of the latter is strikingly illustrated by the difference in the curves.

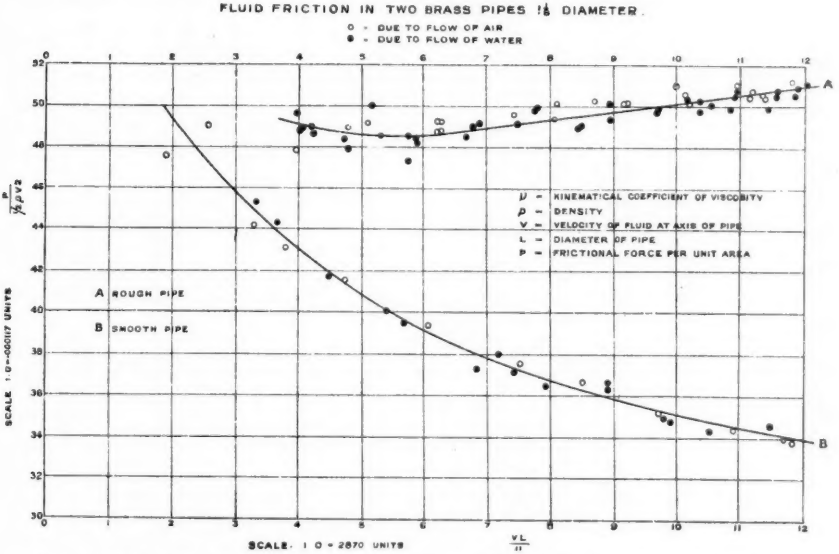

FLUID FRICTION IN TWO BRASS PIPES ⅛ DIAMETER
○ = DUE TO FLOW OF AIR
● = DUE TO FLOW OF WATER
ν = KINEMATICAL COEFFICIENT OF VISCOSITY
ρ = DENSITY
V = VELOCITY OF FLUID AT AXIS OF PIPE
L = DIAMETER OF PIPE
P = FRICTIONAL FORCE PER UNIT AREA
A ROUGH PIPE
B SMOOTH PIPE
A
B
SCALE 1·0 = 2870 UNITS

FIG. 9.—THE SAME OBSERVATIONS AS ARE USED IN FIG. 8, BUT PLOTTED IN ACCORDANCE WITH THE FRICTION LAW OF CORRESPONDING SPEEDS

For the pipes we see that the curves have no apparent connection when plotted at the same speed for air and water flowing through the same pipe. At corresponding speeds they could not be separated without the help of distinguishing points (Figs. 8 and 9).

For wires the lines at the same speed basis have different inclinations and do not always pass through the origin (Fig. 10). On a corresponding speed basis (Fig. 11), however, all the observations fall on a single curve without exception. In either case we could use the corresponding speed curves for all sorts of diameters, all sorts of speeds, and all sorts of fluids. The question which we set ourselves to answer at the beginning of this paper is solved when we know any one of these curves from the model to the full scale machine. To do this is a somewhat difficult task in some cases, and all accurate full scale experiments will be useful in order to establish model testing in a firm and unshakeable position; until then it appears to be impossible to do anything better than the predictions made from models, using a discretion derived from experience on actual flying machines.

## Discussion

Sir G. Greenhill, in opening the discussion, congratulated Mr. Bairstow on the manner in which he had applied the Law of Similarity. This law was, of course, also applicable to the animal world. We saw, for example, the relative disproportion of supporting surface to weight in widely separated flying animals such as the birds and insects. It was interesting to apply the Law of Similarity to the conventional angel. We saw a figure depicted with wings of such a proportionate size that it could not possibly fly. In fact, if we applied the Law of Similarity we should find that an angel ought to be about the size of a bee. It was a great pity that we could not carry out more of our experiments on full size machines in the open air, but failing this we had to have recourse to models, and here the Law of Similarity, which Mr. Bairstow had explained, came in admirably to help us.

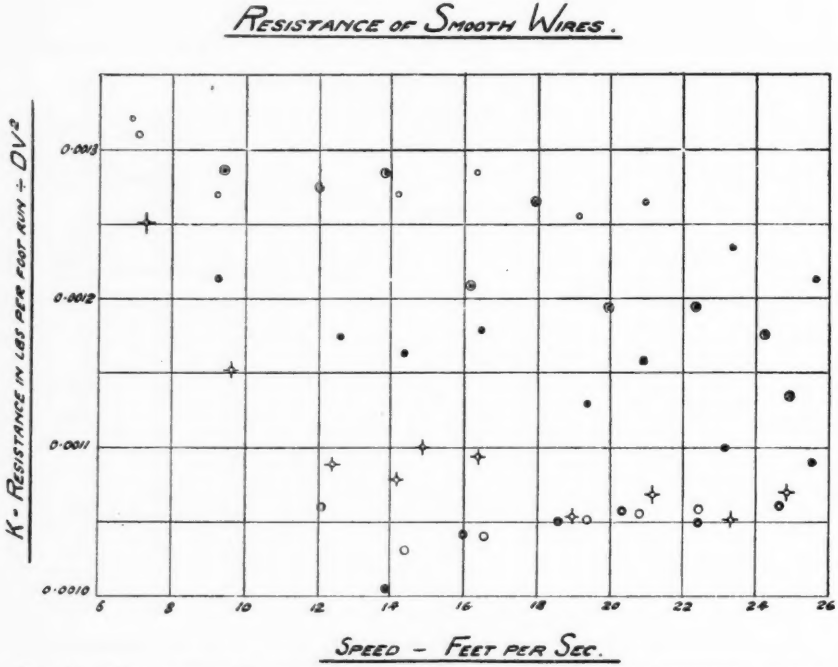



Fig. 10.—Experimental Results on Smooth Wires Plotted Without Reference to the Friction Law of Corresponding Speeds

Col. Fullerton, R.E.: The general principles explained by the lecturer and the system of using the dimensional equations are, of course, correct, but it seems to me to be very difficult to apply them practically.

In the case of the dirigible calculated for the Report of the Advisory Committee of Aeronautics, 1912, the total resistance for a BF. 32 type machine 40 ft. in diam. moving with a speed of 40 miles per hour, is given as 320 lbs., 410 lbs., and 600 lbs., according to the system of calculation used. From Mr. O'Gorman's paper in the same report it would appear to be about 570 lbs. Which result does the lecturer think correct? Again, from the equations it appears to be necessary to make

$$\frac{v^2}{l} = \frac{V^2}{L} \text{ or } \frac{V}{v} = \sqrt{\frac{L}{l}} \text{ and } \frac{vl}{\nu} = \frac{VL}{\nu} \quad \frac{V}{v} = \frac{L}{l}$$

which is not possible. Can the equations be made to agree by using different fluids, allowing also, of course, for the difference of densities ?

There are several other uses to which these dimensional equations can be put, such as finding the dimensions of turning circles, the distance in which a machine started upside-down (or any other way) can right itself, and so on. Perhaps the lecturer will kindly give us his views on this subject. As perhaps some members are not very well used to dimensional equations, I may mention that there is a very useful little book in the Patent Office Library called " Units," by M. Maclean, published 1896, which explains the principles involved very clearly.

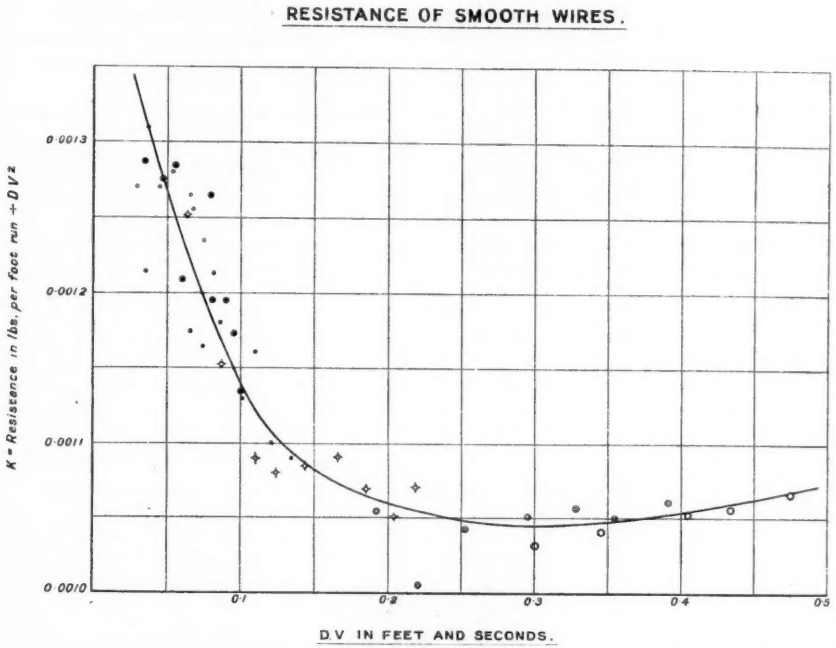



FIG. 11.—EXPERIMENTAL RESULTS ON WIRE ROPES PLOTTED IN ACCORDANCE WITH THE FRICTION LAW OF CORRESPONDING SPEEDS. (Identically the same observations as in Fig. 10.)

Mr. GORDON asked whether it was to be understood that the lecturer treated the air as incompressible, ignoring its elasticity.

Messrs. LOW, HANDLEY PAGE, and THURSTON also spoke.

Replying to Col. Fullerton, Mr. BAIRSTOW said that in his opinion of the figures given for the resistance of an airship of BF. 32 type, the value 320 is the best. He feared that the form in which the figures were originally presented had tended to obscure this preference. The only full scale experiments of which he was aware were carried out on the rigid airship " Schwaben " by the Zeppelin Company, and tend to the same conclusion by comparison made with model tests at Goettingin.

Col. Fullerton referred to the impossibility in certain experiments, probably those relating to the resistance of ship models, of satisfying the requirements of dynamical similarity. He (Mr. Bairstow) was in complete agreement, and although theoretically the difficulty could be removed by using a second liquid the practical difficulties were almost insuperable. The practical approximation used in naval architecture appeared to satisfy the requirements of shipbuilders, and illustrated the necessity for flexibility of method.

It was unfortunate, perhaps, that the laws of dynamical similarity were difficult to apply. When this was the case, however, it was only one way of saying that the problem attacked was difficult. Since the laws must be true it was necessary to face the consequences, and a clear statement of the problem appeared to be a natural starting point.

The cases to which the principles might be applied are probably innumerable, and were to be met with in many scientific works by Lord Rayleigh and others. The solutions to the problems proposed by Col. Fullerton depended on other things than size, and until more detail were given, nothing useful could be added to the formulae of the paper.

He certainly wished it to be understood that he considered the air as incompressible for most aeronautical purposes. On the other hand, this was not the case for all movements of the air, as for instance in the use which was made of it in the petrol motor. It was not easy to show in a general way how little the air density was altered by the passage of an aeroplane, but up to 100 miles an hour the density was not anywhere increased to the extent of one per cent. Perhaps this would answer Mr. Gordon's query.

The CHAIRMAN proposed a hearty vote of thanks to the lecturer for his able and instructive lecture, which was carried with applause.

A vote of thanks to the Chairman, proposed by Col. RAWSON, was carried unanimously with acclamation, and the meeting terminated.

[For the loan of the blocks from which Figs. 3, 4, 6, and 7, are printed, the Society is indebted to the courtesy of H.M. Stationery Office.]

THE SEVENTH MEETING of the Forty-eighth Session of the Aeronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 26th February, 1913, at 8.30 p.m. General Sir John French, G.C.B., &c., Chief of the Imperial General Staff, presided.

# MILITARY AVIATION

By Major F. H. Sykes,
Commandant, Military Wing, Royal Flying Corps.

Sir John, Ladies and Gentlemen,—When, a few days ago, I had finished the notes for this evening, I rather wondered which portion of me, if any, you would kindly allow to leave this room. You will, I am afraid, tell me it is not a lecture on Military Aviation at all, that—but I will not take the words out of your mouth. My excuse is that the words "military" and "aviation" are each so large and lead in such innumerable directions. It is difficult to obtain a correct perspective of the combination. I must rely upon the usual hope of the diffident speaker and say how desirous I am that my efforts may at all events lead to a brilliant and illuminating discussion.

We have recently listened to most excellent technical lectures on subjects to do with the actual design of aircraft. I think it may now be of interest to consider the more military aspect, that is to say the directions in which aircraft will be used and the results to be gained.

## Aviation will not Revolutionise Warfare

It is sometimes said that aviation will revolutionise warfare, or even stop it altogether. This of course is absurd. The main principles of war have been the same for centuries and will probably remain so for several more. Their instruments (of which aviation is the latest and to me the most wonderful), it is which vary. That it will, however, have a great effect on warfare I am convinced. I hope to show the form which I think this effect will take.

## Main Principles of Strategy

First let me attempt to reduce to a few words the great principles of war strategy (as against peace strategy or all the preparations made before war breaks out) which the new arm must attempt to serve; they are these.

*Strategic penetration:* child's play, go for the enemy in his centre: hold him on one hand: beat him quickly on the other.

*Interception:* walk round and masticate him thoroughly from behind.

*Concentration of superior force at the decisive point:* select the hostile weakest point, his flank or rear if possible: mass there: and reap the well-earned results.

It sounds very simple.

But perhaps the enemy may be trying the same methods on you. Roads may be feet deep in mud, railways broken, bridges blown up, passes blocked, rivers (of course quite unfairly) in flood, transport broken down, supplies lacking, men hungry, cold, worn out, diseased. Your generals and staff not made of radium or be of the clock-working omniscient type you have, of course, a right to expect.

Such things have been.

Your countrymen, legislative bodies, press, stock exchange, will take good care that you hear their comments: "Why *doesn't* old Brown-Jones get on?" "What *is* he doing?" "It is all *so* straightforward." "*Can't* the muddlepate see?" "Unless he bags the enemy's army at once we really *must* send Jones-Brown."

War, like most other things, is simple, unless you know something about it.

But how does this affect Military Aviation? In this way. The fundamental difficulties of war are much the same now as they were in the days of Cæsar. Even aviation will not alter them.

In the 13th century armies were of some 30,000 men, and started real business when a few yards apart.

At Waterloo the sides roughly were 120,000 and 90,000.

In 1904, in Manchuria, one battle—Mukden—had a frontage of 80 miles, and the forces in the field numbered about 310,000 each.

In future (except possibly in the case of England) the entire able manhood of the country will take part at one point or another.

Yet in all cases, given more or less an equal degree of preparation, both sides labour under much the same difficulties and enjoy similar advantages.

The old, old principles prevail.

## Development of Instruments and Factors

Instruments, however, change. Factors develop. The latest—aerial work—is pushing its way amongst the innumerable concomitants of war. What will be its effect? Will one side gain? Will both gain, and so re-balance one another?

In any case no revolution of methods will occur.

## General Effect of Aviation on Strategy and Tactics

Before dealing in detail with points which go to make up military aviation, I should like to show briefly their general result.

In the first place the plans drawn out previously in peace will require even greater care and brain power devoted to them in order that the preliminary dispositions of troops, that is the "strategical deployment," are the best possible. An all-round speeding up of the strategic operations may be expected. The sequence, order, counter order and disorder should be less frequent. If the huge masses of modern armies are found to have been wrongly placed no amount of zeal, training, bravery, or mobility can make up. There will be no time for a general re-shuffling. The offensive will increase in advantage over the defensive. Leaders must be prompt and correct in decision. Troops prepared to make long and rapid movements. Army corps will take upon themselves more the role of Naval Squadrons—their positions, strengths and movements will be generally known. True efficiency, leadership of men, the greatest mobility and, other things being equal, the greatest number, will win. Hitherto it has been possible for a small, exceedingly mobile and well-handled force by rapid hidden movements sometimes to defeat considerably larger numbers. It was in this way that the splendid daring of Jackson, the American General, had such great results in the Shenandoah Valley, in the war between the North and South.

*Aircraft will, I think, render this line of action practically impossible.*

There can be no doubt that unless one side definitely obtains command of the air (and I will touch on this in more detail later), the cards will be more openly displayed for both. The problems of land warfare which—if I may say so as a soldier—are far more difficult than those with which my brother officers of the Navy have to deal, will tend, owing to aerial developments, to approximate more closely to those of naval operations. I may say that I look forward to this with considerable satisfaction. We shall still have the enormous strains of mobilisation, of supply difficulties, of lines of communication, of weary legs, but the "fog of war," the "hill" behind which Wellington could not see, will, to a certain extent, be quietly and quickly removed.

Jackson said that "to mystify, mislead, and surprise his enemy is a commander's great object in war." Would such a move as Blucher's from Ligny to Waterloo now be possible? The "Fog of War" was about Napoleon. His plans were upset. Again, at Mukden, the Japanese caused the Russians to think their right flank would be turned. Reserves were hurried east to counteract the expected movement. Countermarching westwards, they could only be thrown in piecemeal against the true Japanese flank attack.

At the time of Mukden the Russians were gradually drawing on their large reserve strength in Europe and becoming adequate to their task—the Japanese were almost at the end of their tether.

How might the world's history have been altered had the Russians, possessing command of the air, and locating the actual movements of the Japanese reserves, met flank attack with outflanking movement ?

Owing to the fear of moving troops in a wrong direction, and having to countermarch them, there will, I think, be a tendency both in the strategical and tactical stages for commanders to await the reports of their aerial reconnoitrers before deciding what to do. Preliminary orders will be issued and confirmed or altered in accordance with the results of reconnaissances.

As the strategical merges into the tactical phase, so the character of the reconnaissance work will be modified. Certain long distance flights will still be advisable to discover possible flanking and reserve movements, but the greater number will consist of short flights to ascertain the tactical position and place the information *immediately* in the hands of the commander.

But can both sides rely upon obtaining such aerial reports ?

So far the results of aerial work have been to do almost entirely with reconnaissance.

In future it is perhaps unfair to assume that one side will have aircraft and not the other. How much will be possible while the enemy is also in possession of an aerial squadron ?

Neither Tripoli nor the Balkans is a guide. The Italians had the air to themselves. The Allies also have been free from interference, except that a good deal of shooting has been done from the ground.

## Command of the Air

General Grierson has told us that war is impossible without command of the air. I am glad that this statement has caused many people to pause and think. But, if I may say so, though I agree with General Grierson with reference to the war of a few years hence, as regards that of to-day, I am not quite so certain. I even hold that command of the air can never really be of the same nature as command of the sea. Neither can the same extent of strategical or tactical freedom in the area of operations be obtained, which might result from the vigorous use of good cavalry.

At sea and on land there are only two dimensions. In the air the third (climbing) is the difficulty. It may, of course, be overcome. We have the precedent of naval evolution from galley to dreadnought. Weight and speed, the problems of naval designers for centuries, are those of aircraft engineers to-day. The enormous strides which aircraft have made during the last three or four years will, I feel sure, be far greater in the near future.

Nevertheless, I feel that the third dimension is a severe stumbling block. A fighting machine with its passenger, gun, ammunition and possibly light armour is a heavy machine. Every attribute is affected. It cannot, for some time, be as fast or easy to handle as an unarmed craft. It will climb more slowly, cause more strain on the pilot, and land with less certainty of remaining whole.

The difficulties may be circumvented. Many clever designers are working on the problem of an efficient fighting aeroplane.

It is sometimes argued that, possibly, it is most advisable at present primarily to develop the number of high speed machines and the training of flyers to handle them. For the time being it would certainly seem that the fast scouting machine will have various advantages over the heavier type with the result that, if both sides use it, both sides will know a great deal as to what the opponent is doing. If both sides also have fighting machines the side upon which this fact has the least moral effect will have an important advantage. A little fighting in the air will, I think, have a far-reaching deterrent effect on the moral of the aerial forces of the losing side.

Military aviation is and must be dangerous. Those who take it up feel its enormous possibilities for success to their side. They accept its risks. The aircraft of one side will be imbued with greater staying powers, greater determination to fight. This side

must be ours. It is this spirit which, creating moral ascendency, always wins on land or sea. It will do so in the air.

Thus again, as usual, we come to the man, the numbers of him available, his patriotism, self-sacrifice and training.

The indications point, then, to two lines of action being attempted by aircraft in war.

The results of reconnaissance work to date demonstrate that each side must attempt not only to gain information, but also to frustrate similar hostile effort. Certain aircraft will be employed purely for scouting purposes, others in fighting off the opposing aeroplanes and airships.

The attempt to obtain command of the air will probably take place duringthe strategical concentration and before land hostilities have commenced. It is improbable that superiority once gained will be much affected by fresh machines being sent to the front. The moral effect accruing from original physical success in the air will be too great.

The side which loses command of the air will labour under all the disadvantages of defensive action.

**Effect of Aviation on Employment of Various Arms**

There has been much discussion as to the effect of aviation on the employment of the various arms. Infantry is of course the arm upon which ultimate success depends. Aviation takes its place with its great auxiliaries. Its alliance is closest with cavalry, and it affects the action of the masses of an army because it influences the uses to which cavalry is put.

Those anxious to reduce expenditure, argue that as aircraft can reconnoitre well the value of cavalry has ceased to exist. This, I think, is quite unsound.

Aircraft will aid and save the cavalry much unnecessary work. Cavalry on its side can help aircraft in many ways. The commander will be fortunate who has the most actively co-operating, highly organised, equipped and trained cavalry and air services. An instance of the value of joint action was afforded during the last manœuvres, when a patrol discovered the outpost line of a division and an aeroplane its transport, and thus, though it was not exactly located, the approximate position of the main body.

The value of information is in proportion to the speed with which it is handed in.

Under reasonable conditions of weather and country a General can now within 3½ hours expect a report as to the approximate strength, formation and direction of movement of the enemy, if he is within an 80 miles radius. A similar result would take officers' patrols sent out from the strategic cavalry at least three days, while the prospects of acquiring the information would be less.

Tactically the aeroplane is ready to undertake a reconnaissance of, say, three hours' duration, whether to obtain information of the enemy's position and movements, to ascertain the nature of the ground to the front, flanks and rear of a position, and to find suitable targets for the artillery.

It will help in the service of inter-communication, in the co-operation of all arms, and also to supplement the telegraph and telephone services in obtaining news of what is happening during a battle.

Moltke's maxim of " march dispersed, fight concentrated " will be aided, a too early deployment and its attendant loss of strength be obviated. The reports of aircraft will afford a degree of security, a saving of officers, men and horseflesh, in anxiety and strain on the commander, in mental wear and tear of the infantry and artillery.

A weaker cavalry better helped by its aircraft may locate an enemy's cavalry, surprise and fight him on ground best suited to itself, and thus clear the way for the infantry main columns. The cavalry will be available to help the infantry in the decisive battle.

When opposing troops are close together aircraft will probably be detached to work with units such as divisions in order that the information may reach the hands of the subordinate commanders immediately concerned as rapidly as possible.

Lastly, we must always remember the great gain in *moral* which the side with the best air service will obtain.

Nevertheless, too much reliance must not be placed on aircraft. The impossibility of work in fog, at night, and in high winds must be borne in mind.

Further, the aircraft reconnaissance is essentially a rapid one. It passes and returns, its field of observation is not very detailed. Small bodies of troops will probably quickly learn how best to hide themselves in the nearest cover, such as woods, villages, &c.

### Recognising Aircraft

Both with respect to fighting in the air and to firing at them from the ground the recognising of aircraft is a difficult question. Those who are accustomed to seeing aeroplanes can often tell to which side or country they belong by their type. A reduction of the number of types used will help in this direction.

Tables showing types of both friend and foe, as seen from below, will probably have to be issued to staffs and troops taking the field.

In future, possibly, aircraft will tend to develop on nationally characteristic lines in the same way as warships have done, but as yet there is very little guide even in this way.

Most British and French aeroplanes are very similar. German ones are, certainly, already somewhat different and more easily recognisable.

The colour of machines, except occasionally in certain lights, cannot be distinguished if they are at a height of over 2,000 feet.

These facts render it a matter of great difficulty to arrange a system of umpiring on manœuvres by means of which an indication may be made as to the advantage gained by one or the other side in the question of air superiority. The naval method whereby two ships speak to each other by wireless and decide any point is obviously impossible. Nor does the system of firing a rocket to indicate to an aircraft that it is out of action, and will not be allowed to continue its work for a time, seem satisfactory. Last year, at all events, our red and blue aircraft had to pass one another, and it is a curious fact worth noticing that, owing to the attention of pilots and observers being concentrated purely upon obtaining information as to the position and movement of the hostile land forces, they seldom even saw each other in the air.

On manœuvres a further condition of unreality is introduced by the fact that aircraft are seldom fired at from the ground. This is probably due to disinclination to shoot owing to difficulty in distinguishing friend from foe, lack of experience in judging heights —experiments with range-taking instruments to determine the heights of aeroplanes have as yet given poor results—the uncertainty of effecting useful results as against disclosing one's position to the aerial observer, and the possibility of danger to friendly troops by such fire.

### Altitudes maintained during Reconnaissances

The state of the atmosphere is mainly responsible for the height at which the reconnaissance of aircraft should be carried out.

Observation is often difficult owing to the clouds and mist, and there is sometimes a tendency to descend to dangerously low altitudes in order to ensure correct information or verify that already gained. Bullets will probably quickly right this tendency in war. I understand that the Bulgarian flyers think anything under 4,000 feet unsafe from fire. Bullets, however, must not cause flyers to err on the side of caution when *looking* for information. The possibility of shrapnel is no excuse for failure. Once *obtained*, results cannot be too jealously guarded. Pilots must in any case endeavour to take advantage of clouds for concealment while minimising their hindrance to observation. On a clear day observations can be accurately plotted on a map from an altitude of 4,500 feet, at which height the ground seems to be moving very slowly, and reconnaissance is relatively easy.

### Pilots and Observers

There is no doubt that the work of piloting and observing entails heavy strain. Battling for even twenty minutes with a heavy machine through a difficult wind is an exhausting task, and commanders must study economy in the use of aircraft at their

disposal. As regards wind, I think it is fair to assume that aeroplanes will be able to fly five days out of six at one time or other of the day. The same pilot and observer should always work together if possible. Speaking tubes are useful between the two. For continuous work two officers per aeroplane are advisable.

Under present conditions and for any considerable period, even in fair weather, it may be estimated that pilots and observers can only be employed for about three hours during the day, or say ten hours in three days. On completing his task, a pilot must, if possible, be given complete rest. If the wind is tricky, the cold intense, or there are other unfavourable circumstances, the above estimate will probably have to be reduced. A reserve of pilots and, possibly of observers, is therefore necessary.

This, however, would mean that observers will as a rule be officers of the Royal Flying Corps or of other regiments, not staff officers. On the other hand, there has been found a difficulty on the Continent in obtaining really good observation officers, other than from those on the staff.

Much careful training and practice will be required whatever officer is selected; not only is considerable experience in the air in seventy to eighty miles an hour machines necessary, but also a large and sound knowledge of military matters. The observer must know instinctively which facts are of importance and which are useless. The untrained officer is of no use. In my opinion the best staff officers, and as many of them as possible, should be trained and kept in practice for this purpose.

### Notes on Reconnaissance

In strategical reconnaissance it is generally a matter of observing the enemy's main bodies. When they are in column of route it is comparatively easy to recognise their nature and to estimate their strength, while in dry weather the dust they throw up gives an early indication of their presence. The position and movements of the hostile cavalry masses if discovered will probably disclose the enemy's intention.

In tactical reconnaissance, on the other hand, troops must be observed after they have left the roads and they are then harder to find; it is most difficult to estimate their strength, and the results must be even more rapidly obtained and communicated. As observers on these occasions, therefore, it is advisable that staff officers, skilled in the work and fully acquainted with the latest reports, should make ascents from time to time in order to gauge matters, watch for movements of reserves or reinforcements, and report immediately to their Generals.

Thus strategical reconnaissance is the easier of the two and will generally give better and more accurate results. Here too, however, careful and constant practice will be necessary, and indeed eventually the training of the observer will probably require even more care than that of the pilot.

Over easy country, that is when there are few but easily distinguished roads and railways, it has been found that the best results are obtained by mapping out a definite course for each aeroplane, for the pilot to find his way and the observer to confine his attention strictly to observing. But in the case of difficult country, when there are many winding small roads, it is usually better to give a definite objective and let the observer direct the pilot.

Over easy country a pilot observer in a single seater machine may be able to gather useful information, especially if it is a question of large strategical movements, but over a difficult area important results cannot be expected unless a definite objective only is given him.

### Night Work

The questions of night marches and of the concealment of troops are of much interest to the observer. It would certainly seem that one of the results of the introduction of aircraft will be more night advances and movements, either to make up for delay occasioned during daylight, to escape notice, or for the purpose of tactical re-shuffling necessitated by the reports of the evening reconnaissance. It has been found that, even in wooded country, large bodies of troops can be seen if they are on the move. It

is of course difficult to report their number, and if halted it is practically impossible to find them. Concealment has been proved to be possible. Various methods of hiding troops must therefore be studied, and may on occasion be effective in war. But if forces are to be concealed and only move at night or in bad weather there will be much delay. Marches and operations will be greatly impeded. In this, as in so many matters, a compromise will probably be made. It will sometimes be advisable to hide, sometimes to push on and chance being seen. It is a question of generalship. Night marches, when troops are weary and short of food, are not popular; they entail heavy wear and tear, and if continued for long lower the fighting metal of the men.

During last manœuvres neither side did much in the way of night marches, owing probably to the desire to keep the troops fresh for a final effort. If a night march *is* to be undertaken, no preparations should be made which it will be possible for aircraft to see.

Not much has been done by aeroplanes at night, but such work may, I think, be considered one of the most important duties of airships. It is of considerable difficulty and requires much practice and training.

I am afraid, therefore, that the unfortunate soldier must not expect to be free from the baneful influence of aircraft at night. His bivouac cooking-fires—when and in what number to use them—will have to be considered, and the fact remembered that they should not be left burning in the morning.

The danger to aeroplanes attempting to land at night is still very considerable. Machines sometimes return late however, and land when it is almost dark with the help of petrol flares.

Troops billeted in villages or towns will probably be an uncomfortable problem for the observer. It will undoubtedly be very difficult to estimate their strength.

### Handing in Information

Having obtained information the greatest value must at once be gained from it. To effect this the commander of the aircraft must be in constant touch with the General Staff. His observers should be placed in full possession of all information already gained and movements intended. An aircraft may happen to spot a hostile body in quite an unexpected locality, and, if it appreciates the situation, rightly decides to return and report this before going on to its allotted task. The freest hand possible should be allowed. It will usually be advisable to send two aircraft if the mission is a very important one.

### Selection of Landing Places

In selecting a new position for Army or other Headquarters to which aeroplanes are attached, the necessity of having a suitable adjacent landing ground should be given considerable weight. Staff Officers could quite easily learn the type of ground required. Routine instructions have already been drawn up.

### Service of Intercommunication

Owing to the character of the country it may often be impossible for aeroplanes to land quite close to Headquarters. A considerable advance has been made in methods of dropping messages but much practice is yet required in this direction. Motor cyclists as despatch carriers are most useful. When the aircraft have inevitably to work from a point distant from Army Headquarters motor cars are used for observers personally to report to Headquarters for instructions before starting, or on return to supplement written reports by verbal information.

The question of signals from aircraft is a complex one. Lights, puffs, discs, Klaxon horns, &c., have been tried, but the results have, so far, not been entirely satisfactory.

The employment of an aeroplane for the transmission of intelligence from Army Headquarters to the Cavalry Division has been found to answer well.

### Wireless Telegraphy

The French are reported to have sent messages a distance of 50—60 miles by wireless. The difficulty of receiving owing to noise both in airships and aeroplanes has yet to be overcome, and the interference between stations also restricts its use. If used the necessity for a cipher is obvious, and only a few important messages should be sent.

### Accessories

Many people are astonished at the apparently extraordinary number of accessories required to keep a number of aircraft in the field. Transport, spare parts, tools, sheds, mooring masts and other absolutely innumerable impedimenta. On manœuvres last year some eight motor cars, twelve light tenders, ten heavy tenders, and eight Foden steam lorries were fully employed to keep two airships and fourteen aeroplanes going.

May I here interject a hope that constructors will recognise our difficulties in this respect and give all the help they can towards some degree of standardisation.

### Casualties

During prolonged operations it will be a matter of great difficulty to maintain more than 50 per cent. of machines in working order, and even this will necessitate a great quantity of immediately available spare parts, and a high degree of training in all concerned.

It would, I think, seem probable that no aeroplanes or engines, and few pilots and observers, will last more than three or four months on active service.

Efficient and sufficient repair lorries are essential, though present experience points to the fact that first-aid repairs can only be of the character of replacing damaged parts. More serious damage must be repaired at the Flying Depot at the advanced base.

A considerable source of wastage will probably lie in the fact that engine failure will have an inconvenient habit of occurring when a machine is securing its very best information over hostile country ! Even the Royal Aircraft Factory cannot help us under these circumstances ! The best remedy lies with engine manufacturers, and it is in this direction we hope for the next great step.

### Sheds

Sheds if used require a great deal of transport and personnel. Actually many aeroplanes can remain out at night for short periods if some sort of cover is provided. Sheds will probably only be kept for overhaul purposes.

If, however, a shed could be designed which is sufficiently light to permit of one per aeroplane to be carried without prohibitive transport, the efficiency of the machines and their detachments would undoubtedly be much increased.

The method is practicable to use the present sheds but only to move the field base every three or four days. The disadvantages of this procedure are, that the field base will probably not always be with Army Headquarters as it should be. There may be a tendency to loss of touch, and to the waste of valuable time morning and evening.

The weight of airship sheds of course renders them quite prohibitive. Mooring masts and a prayer for good weather have to be put up !

### Transport

Even if sheds are not carried, transport (with its own complement of spares) is essential also to permit of the field bases being moved to points convenient to the landing places of the aeroplanes as, otherwise, valuable time will be wasted in the aeroplanes having to go some distance to the rear for overhaul, supplies of petrol and oil, food for the men, etc., etc.

### Personnel

The technical personnel at the Field Bases requires a high degree of training (in addition to a large proportion of physical and mental steel) and must be in sufficient

numbers to provide reliefs. The bases have frequently to be moved by day and the overhaul of aircraft to be carried out by night.

Now, please let me say a few words with reference to the types of aircraft which I consider are required immediately, then, well I am going to dream for a moment!

To sum up the conclusions to which the many considerations I have tried to place before you this evening lead me, I think we want:—

*First*:—For strategical work, a single-seater scout aeroplane with a speed of 90 miles an hour, a landing speed of half that figure, a very high rate of climbing, and a petrol capacity of say 300 miles. Good view is also essential.

*Second*:—A two-seater with speeds of 80 and 40 and 200 miles tankage; carry a light weapon, be a good climber, and be capable of landing on bad ground. Good view.

*Third*:—A two-seater fighting machine with speeds of 70 and 40, to carry a gun, ammunition, light armour, and petrol for 200 miles. Again of good climbing powers.

*Fourth*:—A semi-rigid airship of about 250,000 cubic feet capacity, a speed of 55 miles an hour, keep the air for at least six hours. To carry a crew of eight, a light gun, wireless, searchlights.

We still in England are rather apt not to recognise the capabilities of airships. They have not yet attained to really great speed but their range of action is very large, observation is easy from them, they can hover silently, carry light armament, drop bombs or explosives, they can fly in quite strong winds and rise at a rapid rate. The answer to such vessels other than meeting like with like is at present difficult to see. They are a very formidable weapon.

It is an unwise satisfaction to shirk such realities.

The types I have mentioned are those wanted now. I dream, in the not far distant future, of scouting aeroplanes of 120 miles an hour; fighters to carry pilot and assistant, gunner and observer at a speed of 100 miles; weight-carriers to transport troops, rations and equipment, ten or twelve at a time, a distance of 30 miles and make five trips a day. Four hundred of these and some twenty to twenty-four thousand men are landed a double march ahead, with no weariness of the flesh, but rather physically and mentally braced up by a pleasant journey. The navies of the world—I am sorry for them—but, in my dream, they have somewhat to relinquish their present proud position; their rôle is that of floating defence, the air service—built up from joint army, navy and civilian foundations—is in the foremost line; fortresses, arsenals, dockyards, Government offices, factories of war material, are protected from the air by an elaborate system of . . . I don't think I will tell you that yet.

How extraordinarily interesting it all is.

## Dissemination of Information

But, for England to maintain her political weight in the world these possibilities, though at present still dreams, must be looked into, worked at, grappled with until the Army and Navy and public understand at least their dangers.

We are so slow at taking up a new thing. Is it that our national imagination is sluggish—that we are wanting in mental alertness? The phrase "the slowness of great strength" is out of date. Strong in courage and knowledge, quick and certain in action, must take its place.

The Aëronautical Society is doing all it can to help. But many more channels for the rapid dissemination of knowledge on aerial subjects are required. If I may say so, I think that if the Royal Flying Corps had done nothing else (and it is doing a good deal) the fact of its having brought the two services into joint action would be quite sufficient justification for its existence.

The public must now be brought into partnership. In France the aerial services have the solid backing of consistent popular opinion. We cannot do without it in this country. With it we can and will take the foremost place in the air as now on the sea. All rests eventually on the public. It must not be allowed to shirk its responsibility.

Information as regards developments in aircraft designs and employment by land or sea, progress in the formation and training of the Royal Flying Corps, lessons learnt in aerial reconnaissance, meteorology, wireless and the vast number of other kindred subjects

must be put in their possession. As a help in this direction we hope shortly to start a Royal Flying Corps Journal. Hearty hospitality will be extended in this to articles on subjects in connection with service aviation. I am not pessimistic as to the question of interest in aeronautics coming. It will come I know, but it must be made by us all to come quickly.

## DISCUSSION

Mr. MERVYN O'GORMAN said that such lectures as the present were invaluable to constructors. Major Sykes was the kind of man whose dreams would be realised—"Infantry," he had said, "was of course the arm upon which ultimate success depends"—and it was his dream to see the aeroplane transporting infantry. That was one step. The next is to make that aeroplane. These were only straightforward difficulties, and half a dozen men in that room could do it in two years from the date of the word "go." The question was, how many years it would take the public to say the word "go."

As to its utility, a modern battle—Mukden, with a frontage of 80 miles was a good example—and since to concentrate a superior force at the decisive point is a main principle of strategy, it is most difficult to conceive of a better way of effecting unexpectedly such a concentration of 30,000 men than silent aerial transport—say 10 men per aeroplane, by three journeys, 1,000 aeroplanes. Mechanical assistance to disembarking and the instant collection of the groups of 10 men to allow the next oncoming aeroplane to alight would have to be studied, but the flights themselves would only be matters of 30 minutes for 30 miles. A single clear night or a misty morning might allow of this move, which would presumably be masked.

He felt interest in noting that the lecturer contrasted the past and future in these terms: "Hitherto it has been possible for a small exceedingly mobile and well-handled force by rapid hidden movements sometimes to defeat considerably larger numbers"—and for the future, "Aircraft will render this line of action practically impossible." It seemed to him (Mr. O'Gorman) that the opposite was likely to be true in many instances, when "Aircraft will render this line of action practically imperative," if we admit of mobility being obtained by aerial transport, and of the command of the air lapsing from day to day.

The near presence of an alighting ground for one force and not for the other would apparently be a matter of appreciable importance for other reasons than scouting.

It would appear that when practicable aerial transport would help to "mystify, mislead and surprise a commander" nearly as much as aerial scouts would clear up his troubles and so help to justify the favourite formula that a new arm does not revolutionise warfare.

The gross weight of three tons, which may be provisionally taken as the limit of aeroplane construction, could doubtless be attained in about two steps from our present 15-cwt. stage, and each step would take a year. Mr. O'Gorman thought this gross weight would allow passably for 10 men with rifles and a little food. It would also allow of carrying a land gun weighing a ton and ammunition, or as a further alternative the load could all be fuel to permit of great air endurance without replenishing. Alighting by night would grow in importance and for it better landing gear than existed at present was wanted, also illumination, brakes, retarding devices, and means of steering on the ground in a following wind so as to utilise restricted areas.

He had hoped for instruction as to the relation between the numbers of aircraft and the numbers of the armies they serve. Was it not possible that a small army might require more so as to make in efficiency what it lacks in numbers? He thought that this might be read into the lecture from the following:—

"The attempt to gain command of the air will probably take place . . . before land hostilities have commenced. It is improbable that superiority once gained will be much affected by fresh machines being sent to the front. The moral effect accruing from the original physical success in the air will be too great. The side which loses command of the air will labour under all the disadvantages of defensive action."

If this be illuminated by the other dictum—"other things being equal the greatest number will win"—it leaves no alternative than to equality *at least* of aerial force,

whatever the disparity of numbers on the ground—a view which commended itself to him, since it is unwise to suppose that *other things* like generalship will be in favour of one's own side.

The saying that offensive action is the best form of defence could be carried into the technical aeronautical world where taking the initiative was analogous to taking the offensive, and the moral is that we do quickly proceed to new developments without awaiting their appearance elsewhere.

The two-engined aeroplane would probably settle the difficulty of engine failures. The claim that aircraft would not revolutionise war was so often repeated as to sound like a dogma and as such to require investigation. It depended whether a change was or is not to be called revolutionary when it altered a war of blind-man's buff in a fog into a war like chess in the sunshine. A revolution might occur, although the banking system, the moral code and the religious authority continued. It also depended on whether both sides adopted the new arm, in which case the result would be less revolutionary, whereas it would be hard to suppose that a fleet of Dreadnoughts would make many elaborate plans to defeat an Armada of wooden boats. For such a case the Dreadnought revolutionised naval warfare. However, he was willing to bow to any abnormal use of the word " revolutionise " from the reader of so excellent a paper which admitted and ascribed to aeroplanes, poor as they are, the speeding up of strategic operations, an increase of advantage to the offensive, more long and rapid night movements, simplification of the problem of land war to the level of naval war, the removal of the fog of war, the necessity for obtaining the command of a new element, the air.

Brig. Gen. D. Henderson : I agree generally with the lecturer, especially in regard to the training of observers, which is most important. When I was at the Staff College we were taught to ride across country. I hated it. There will now have to be a professor of aircraft, and staff officers must be trained in observation from aircraft. There is considerable difficulty in observing from about 4,500 feet and our reconnoitrers must be specially taught observation in special machines, which it is for our designers to produce.

Captain Godfrey Paine, R.N. : I agree generally with Major Sykes, but I do not quite follow the idea of transporting large bodies of men by aeroplane. Say we have 1,000 machines with 30 men in each, how are we going to land them near each other ? It is bad enough when you have about a dozen aeroplanes trying to land in one aerodrome. If the 1,000 machines gave each other plenty of room some of the men would have a long march even when they were landed.

Captain Paine also referred to the great difficulty of landing machines successfully in rough weather or on rough ground, especially at night.

Captain H. F. Wood : The paper we have just heard is difficult to criticise, but I should like to raise one or two questions. With regard to armoured aeroplanes, these could not lift or travel as fast as a smaller machine, but they could be built if speed were sacrificed for the same lift.

Before machines can be standardised, I suggest that the first thing is to standardise requirements. The types asked for in the Military Trials are now departed from, for example, the $4\frac{1}{2}$ hour machine is not now asked for. What constructors are asking for are definite specific tests for them to meet. At present they do not know what is required.

Major-General R. M. Ruck said he was very glad to see Major Sykes emphasising the value of airships, in which he was afraid we were very deficient. He also looked forward to the designing of an efficient fighting aeroplane and would like to see a prize given with that end in view.

Major Sykes had described what he had called a dream, but he, General Ruck, had not had a dream but what he could only call a nightmare, and that was to imagine the Royal Flying Corps pitted against an army with a really adequate equipment of aeroplanes. He was afraid our brave fellows would not have a chance to show their capabilities for lack of machines.

Mr. A. E. BERRIMAN said it would be an incidental advantage if Aeronautics led the general public to take more interest in the Services.

As regards standardisation, while it was early to standardise design, constructional details, such as bolts, propeller flanges, &c., might very well be taken in hand at once. He had not seen it stated in what direction guns on aeroplanes were generally required to fire, up or down.

Mr. S. F. CODY said that he had several times practised night flying and landing, and had come down always without harm. Going up at dusk the eye got used to the fading light as it got darker, and anyone used to the country could nearly always distinguish what sort of ground he was over, that was except on really pitch-dark nights. Personally, he found that lights on the ground had a blinding effect unless they were shielded.

He was sorry to have to talk about what he had himself done, but he was the only man he really knew all about! His first machine had 1,010 square feet of surface and could do all it was wanted to do. He could have carried 10 men a distance of 50 miles if it had had sufficient engine power in it. He believed in the large machine, the bigger the machine the easier it was to drive, in fact, the bigger the better altogether. He was tired of building midgets. He did not propose to criticise the military side of the paper as he was a pilot and constructor. Major Sykes was a pupil of his when he was in charge of the Army Kites. He would like to say that he had been 3,000 feet in the air attached to a kite long before most of those present ever went in a heavier-than-air machine.

Major SYKES then replied briefly to the discussion.

The CHAIRMAN: I have listened, as I am sure you all have, with deep interest to this excellent lecture which affords us ample ground upon which to found clear ideas as to the lines upon which military aircraft will develop.

The Aëronautical Society is to be congratulated on having organised what is to me the most interesting lecture and discussion I have heard in this hall. General Henderson's remarks as to training I cordially agree with, and should like to bring to your notice the excellent example which he set by going and learning to fly and getting his certificate without saying anything about it.

Mr. O'Gorman's remarks as to transport were very much to the point, although Captain Paine's ideas seem to be in conflict with Mr. O'Gorman's.

In my opinion, whilst the three arms still have their same proportional importance it is certain that the method of their employment will be modified by the new conditions imposed by the use of aircraft.

Cavalry will gain much assistance from them in the sphere of strategy and of strategical reconnaissance. Aircraft will be a valuable auxiliary to them and the additional assistance thus afforded will enable them to take part in a more efficient condition in the decisive events of a general action and pursuit.

With regard to the artillery, one can foresee the time when the co-operation of aeroplanes in the choice of targets and in the correction of fire will much increase their efficiency in the field and may also lead to economy in the expenditure of ammunition.

It is much more difficult at the moment to foretell the influence which aircraft will have upon infantry and its methods of fighting excepting so far as the necessity for concealment by day and by night, and great efforts to avoid observation are likely to cause increased hardship.

Perhaps the employment of aircraft will have the most marked effect in the influence which their operations must bring to bear on the minds of the higher commanders in the field.

Owing to modern invention, what we know as the "fog" of war is now more dense than it ever was before, and has made its influence felt in the strongest way throughout the campaigns of the last decade.

The operations of aircraft will make a great change here. Strategic and tactical moves and combinations can no longer be made in secret. Successful attempts at deception which have lately had so favourable an influence upon the side which has known how to use them will lose much of their effect under the close observation of aircraft, and the game will have to be played with the cards on the table far more than has recently been the case.

During a large part of my life I have been a leader and instructor of cavalry. The reason why I have consistently fought, sometimes against great odds, in favour of the *arme blanche*, is because I have ever possessed such an intense faith in the efficacy of the cavalry battle. I have always held that when the two reconnoitring cavalrys come together they must concentrate and fight, because the result of that fight will absolutely dispel the "Fog" of war for that side which is successful and render it tenfold denser for the vanquished.

As time goes on and aircraft become more stable and reliable the tendency of soldiers will be to train and prepare for battles in the air, and it is the result of those marvellous collisions which will, more than anything else, decide the future value of aircraft and to what extent they will influence the wars of the future.

I have spoken at some length on this subject because I consider it one of the most important questions for modern soldiers to study.

The account given by Major Sykes of the progress of military aeronautics and his speculations as to possible future developments are profoundly interesting.

The Royal Flying Corps is a new creation in which officers and men of the Navy, the Army, and direct from civil life are devotedly working in harmony for a common end. They risk their lives every day and many have already sacrificed them to bring about the development of an effective air service for this country, and we owe them a very deep debt of gratitude.

Whilst this development proceeds in the manner outlined by the Commandant of the Military wing and while this splendid spirit continues to prevail I have no doubt whatever as to the immense value of the Corps, and I feel sure it will give the best possible account of itself should it be called upon to take the field. We would all desire to congratulate the Corps on the successful carrying out of their trip to Montrose.

Our warmest thanks are due to Major Sykes for this most interesting and valuable lecture.

Major-General R. M. Ruck proposed a hearty vote of thanks to the Chairman for his kindness in presiding at the meeting. The presence in the chair of the Official Head of the Army was a very great honour, which the Society fully appreciated.

The motion was carried with applause, and the meeting then terminated.

THE EIGHTH MEETING of the Forty-Eighth Session of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 12th March, 1913, at 8.30 p.m. Major-General R. M. Ruck, C.B., presided.

The CHAIRMAN introduced the Lecturer as an old and well-known member of the Society, who had done valuable research work, was still doing it, and who, they all hoped, would continue to do it for many years to come.

# AERONAUTICAL RESEARCH IN THE HOME

BY A. P. THURSTON, D.SC., A.F.AË.S.

Anyone who is worth his salt is fired with the desire to add to the sum of human knowledge. That is one of the basic principles of our lives. For all such there is a vast field of useful work in the science of Aeronautics, provided they have sufficient determination and grit. It is to these I am speaking to-night, and it is my sole purpose to endeavour to direct interest in research work by showing how easy it is to construct simple apparatus with little money. I propose afterwards to describe a few diverse experimental researches made with such an apparatus. Needless to say, it will be a real pleasure to me to give any assistance or advice I can to all who may decide to instal such apparatus. The greater the number of experimenters at work, the faster will our knowledge proceed, and so long as the work is done it matters little whether it is you or I who do it.

The apparatus which I want to describe is shown in Fig. 1. It consists of a small wind tunnel 1 ft. square in section having horizontal and vertical portions. A series of grids, A, may be placed in the tunnel to prevent rotation of the current. The tunnel is conveniently made of three-ply wood. Air is drawn through it at any speed up to 20 m.p.h. by a four-bladed fan mounted on a $\frac{1}{8}$ H.P. electric motor costing about £2. The velocity of the air in the tunnel may be determined by means of a Pitot tube in conjunction with a micromanometer such as that described* by Mr. B. J. P. Roberts and manufactured by J. J. Hicks, 8, 9 and 10, Hatton Garden, W.C.

A suitable Pitot tube is shown in Fig. 1, kindly described to the author by Mr. Harris Booth lately of the National Physical Laboratory. It consists of two concentric tubes one within the other. The inner tube has its aperture flush with the end of the outer tube, and the outer tube is sealed at each end. A ring of five holes 1/50th inch in diameter is bored into the outer tube $2\frac{1}{2}$ inches below the sealed end and a tube connection is let into the outer casing. The projecting end is therefore connected to the dynamic tube and the tube connection with the outer or static tube. The projecting end and tube connection are connected by tubing with the micromanometer shown in Fig. 2. The two limbs, A, $A^1$, of the U-tube are connected by a tube, B, of finer bore which contains an index bubble, C, of air, acting as a piston. The length of the index bubble should be approximately equal to the distance between the centres of the limbs; it will then be found that the gauge is practically indifferent to changes of position or level, as the effect of the air bubble will counteract the effect due to any change of level of the limbs. The sensitiveness of the gauge may be of almost any extent as it depends only on the ratio of the bore of the index tube to that of the upright limbs. The fluid used should have a low surface tension and viscosity. Xylol, having a specific gravity of ·86, is recommended for the less sensitive gauges and petrol for the more sensitive. A suitable gauge for use with the tunnel should have a range of about $\frac{3}{4}$ inch of Xylol.

The velocity is given by the following equation :—

$$v = \sqrt{2g\,h}.$$

Where v is in feet per second

g is the acceleration due to gravity=32·18.

h is the difference of pressure in the two tubes in feet of air at the statical temperature and pressure of the air in the tunnel.

* Proceedings of the Royal Society, Vol. 78.

The various forces on planes and bars may be read by a simple balance similar to the one previously described* by the author, but of smaller scale. This balance should be mounted in a detachable case which may be fixed to the tunnel at either of the positions, B. If the specimen is rotatably mounted on the end of the beam and is provided with a pointer which runs over a graduated quadrant fixed to the beam it is clear that the drag and the lift of inclined planes at like angles may be read respectively in the vertical and horizontal portions of the tunnel. By this means an expensive balance is avoided.

A vast number of useful experiments may be made in this tunnel without expensive alterations.

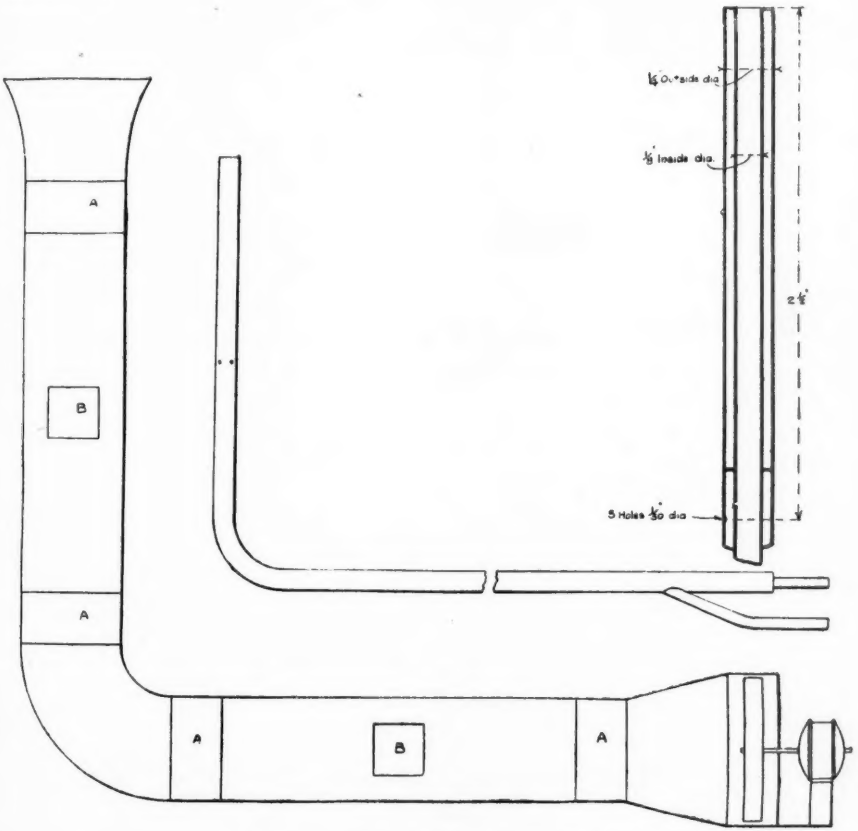



FIG. 1.—WIND TUNNEL AND PITOT TUBE

The following are described by way of example :—

**The distribution of pressure on rods and struts.** If we place a bar in a current of air we know roughly that the air exerts a pressure on the front and a suction on the back. Our knowledge of the flow of air around various bodies would be extended if we knew of the pressure at every point around the body. A hollow circular rod 0·72 inch diameter was first experimented with. The ends were sealed and a small hole 1/50 inch diameter was drilled in the centre. The interior of the tube was connected to one arm of the micromanometer and the other arm to the static tube of a Pitot tube. The pressure of the air at the point occupied by the small hole could therefore be determined. Means

* AËRONAUTICAL JOURNAL, April, 1911.

were provided to allow the rod to be rotated, and a pointer outside the tunnel indicated the number of degrees through which the rod had been rotated. The pressures at the various angles were noted and are plotted on the radial diagram in Fig. 4. Compression is measured radially inwards. A butterfly diagram is thus obtained. A very extraordinary state of affairs is revealed by this diagram. There is a compression on the bar for only 40 degrees on either side of the centre of the side normal to the wind. All the remaining surface of the bar is under suction and, marvellous to relate, the greatest suction is at about 70 degrees on either side of the zero position, *i.e.*, on the half of the rod which faces the current.

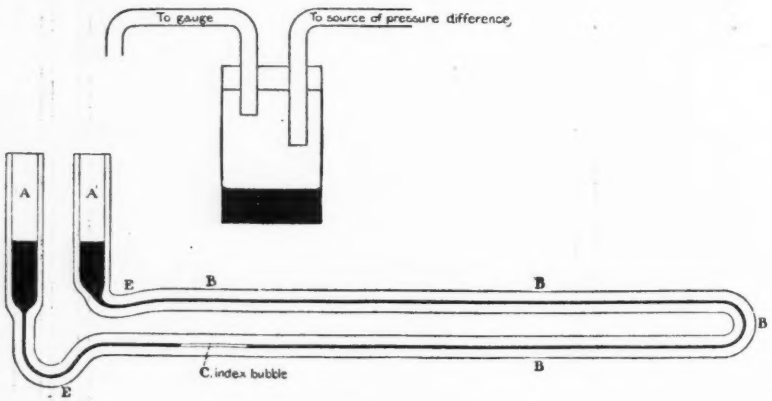



FIG. 2. MICROMANOMETER

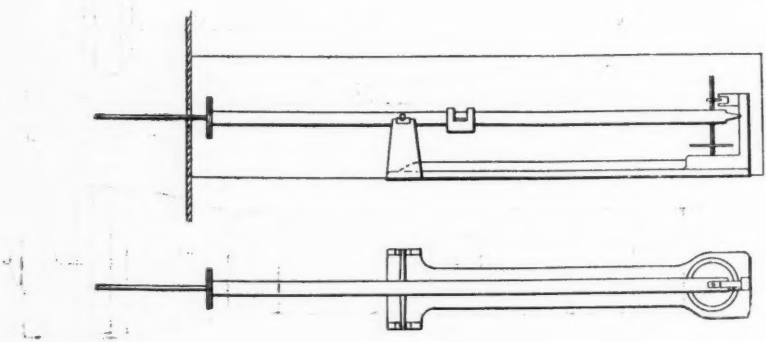


FIG. 3.—BALANCE

The next diagram, Fig. 5, shows the distribution of pressure on the same rod, when another rod of equal diameter is placed directly above and in contact with it. The diagram is not symmetrical, and thus the two bars are obviously not directly in line with the current. As great care was taken in placing the bars the deviation from the correct line cannot account for the want of symmetry. It is rather thought to be due to a slightly recessed glass door in the side of the tunnel causing a disturbance and oscillation in the flow. Despite the want of symmetry the diagram shows clearly that every point of the surface is in suction and that the greatest amount of suction is on the half facing the wind. This bar, therefore, will not require any power to drive it, but will actually

apply a force to the rear of the front bar, helping it to travel forwards against the air. We should expect, therefore, that the total resistance of the two bars in contact would be less than that of the single bar. That this is so, has already been shown by the author in a previously published diagram.* From this diagram it is seen that the total resistance of the two rods in contact is only slightly more than half that of the single rod.

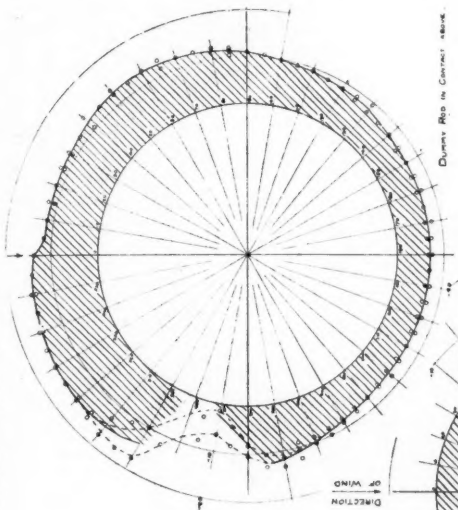



FIG. 5.—DIAGRAM SHOWING THE DISTRIBUTION OF PRESSURE AROUND A CIRCULAR ROD WITH A DUMMY ROD OF THE SAME DIAMETER IN CONTACT ABOVE

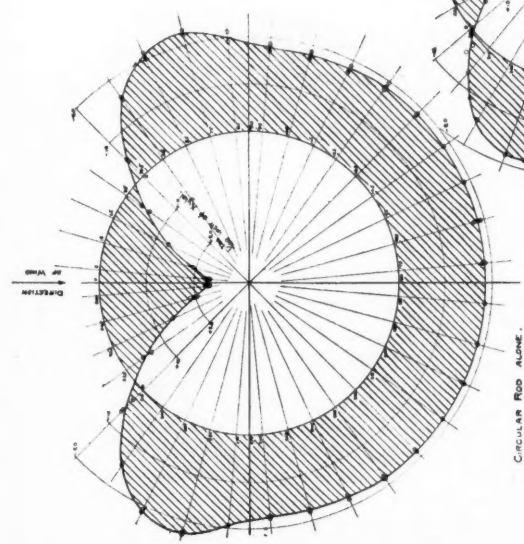



FIG. 4.—DIAGRAM SHOWING THE DISTRIBUTION OF PRESSURE AROUND A CIRCULAR ROD

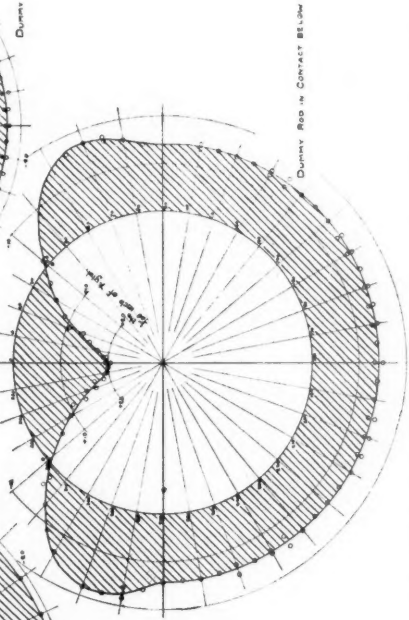
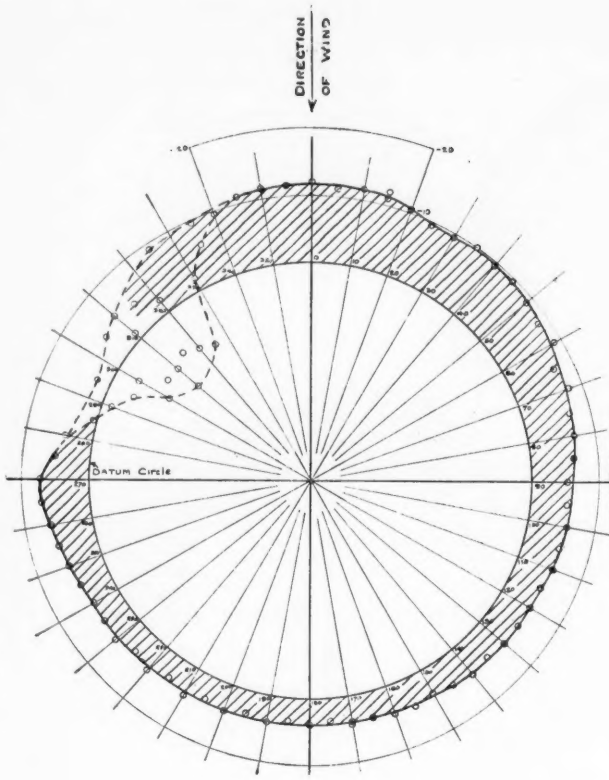


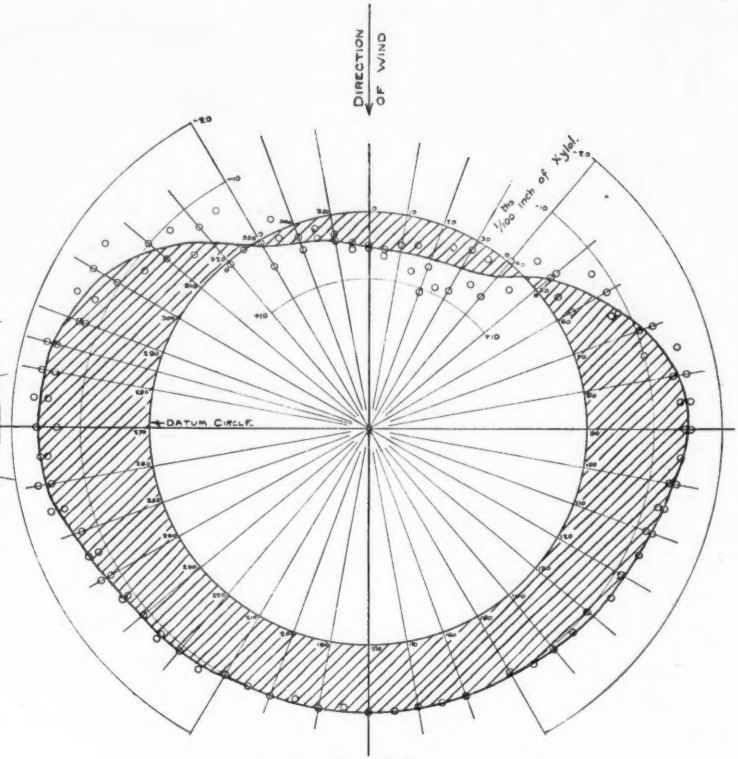

FIG. 6.—DIAGRAM SHOWING THE DISTRIBUTION OF PRESSURE AROUND A CIRCULAR ROD WITH A DUMMY ROD OF THE SAME DIAMETER IN CONTACT BELOW

* AERONAUTICAL JOURNAL, July, 1912, page 184.




DUMMY ROD 1 INCH ABOVE

FIG. 7.—DIAGRAM SHOWING THE DISTRIBUTION OF PRESSURE AROUND A CIRCULAR ROD WITH A DUMMY ROD OF THE SAME DIAMETER, 1 INCH ABOVE




DUMMY ROD 3⅛ INCHES ABOVE

FIG. 8.—AS IN FIG. 7, BUT WITH THE DUMMY ROD, 3⅛ INCHES ABOVE

The dummy rod was moved from its position above the test rod to a similar position below and in contact with it, and a series of readings again taken. A butterfly diagram, Fig. 6, was again obtained which is similar in all its essential features to that shown in Fig. 4, but all the pressures appear to be slightly less.

The wooden dummy rod was then placed 1 inch above the test rod, and the resultant readings are shown plotted in Fig. 7. As in Fig. 5 the diagram is unsymmetrical and the same remarks apply. It will be noticed that, except for the unstable portion of the diagram, the whole surface of the rod is in suction and the greatest suction is on the half of the rod facing towards the direction of the wind. The rod will therefore exert a force in a direction opposite to that of the wind. All the pressures appear to be less than in the corresponding diagram shown in Fig. 5.

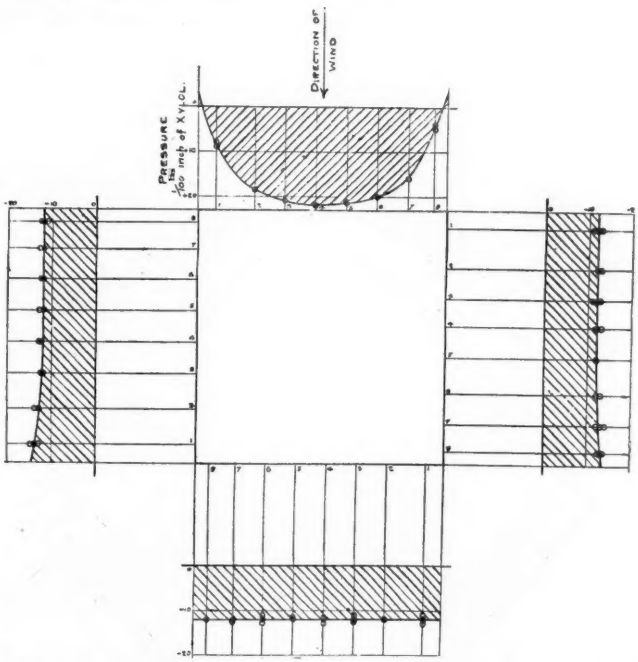



FIG. 9.—DIAGRAM SHOWING THE DISTRIBUTION OF PRESSURE AROUND A SQUARE ROD IN NORMAL PRESENTATION

The next diagram, Fig. 8, was taken with the dummy rod 3⅛ inches in front of the test rod. The rudiments of the butterfly diagram of Fig. 4 are appearing. A portion of the front half of the diagram subtending an angle of 78 degrees is in compression and all the rest of the surface is in suction, the greatest suction appearing to be on the portion of the surface facing the wind. The rod in this position clearly exerts a drag in addition to the drag on the front bar and explains the reason for the quick rise in resistance of two rods as the gap between is increased. The experiments with circular rods may obviously be continued with useful results in many directions. The next diagrams, Figs. 9 and 10, show the distribution of pressure on a hollow square bar with the sides and the diagonal at right angles respectively to the wind.

In the first case, Fig. 9, the side normal to the wind is in compression except for small portions at the outer edges which would appear to be in suction. The greatest compression is at the centre. The sides which are tangential to the wind are in suction over the whole surface, the suction increasing slightly from the front to the back edge

and reaching a maximum at the back. The side away from the wind appears to be under a uniform suction which is about equal to that of the front portions of the side faces. In the second case, Fig. 10, with the diagonal normal to the wind the two sides towards the wind are in compression for about three-quarters of the width from the entering edge and the remaining quarters are in suction. The greatest compressions are at the entering edges and the greatest suction at the opposite edges. The sides away from the wind are in approximately uniform suction.

Figs. 11, 12 and 13 show corresponding diagrams for a hollow elliptical bar. It should be stated that the pressures at the various points in the elliptical as in the square bars were obtained by drilling a series of fine holes in the bar, stopping them with wax and unstopping each hole in turn. In Fig. 11 the major axis is parallel with the wind. There is considerable pressure at the front which rapidly falls off. A very slight compression remains along the sides and appears to extend beyond the mid section. There

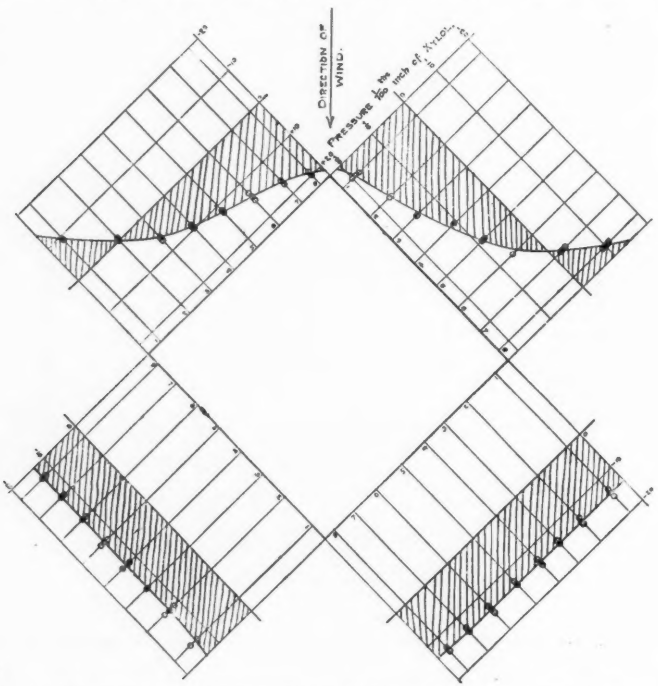



FIG. 10.—DIAGRAM SHOWING THE DISTRIBUTION OF PRESSURE AROUND A SQUARE ROD IN DIAGONAL PRESENTATION

is a slight suction at the rear which is not so great as the corresponding compression at the front. In Fig. 12 the same bar is shown with the major axis perpendicular to the wind. The compression on the entering side is greatest at the middle and extends over a considerable portion of the side. As with the square bar in diagonal disposition, the outer portion of the entering side is in suction which extends entirely around the back.

In Fig. 13 the major axis is at an angle of 45 degrees to the wind. The compression on the windward side is now unsymmetrical and the greater portion is in front of the minor axis. The whole of the leeward side is in suction which is considerably less than the compression.

The author gratefully records his indebtedness to Mr. H. Bennett for erecting a wind tunnel similar to that described and for making the readings used in plotting the distribution of pressure in the previous figures.

**Apparatus for determining the travel of the centre of pressure, the control torque, and the longitudinal stability coefficient of various plan forms of aeroplanes.**

If XY (Fig. 14) is the longitudinal axis of an aeroplane, and the forces L, L$^1$ acting at the points *a*, *b*, are determined by means of the two pivoted balances or beams, then it is obvious that the position of the result R is readily determined. If, further, the longitudinal axis is swung into various inclinations to the current, it is obvious that the position of the centre of pressure at these angles may be determined and the results plotted in the usual way. This is the principle of the apparatus shown in Fig. 15, which may be used to determine the travel of the centre of pressure, the control torque, and

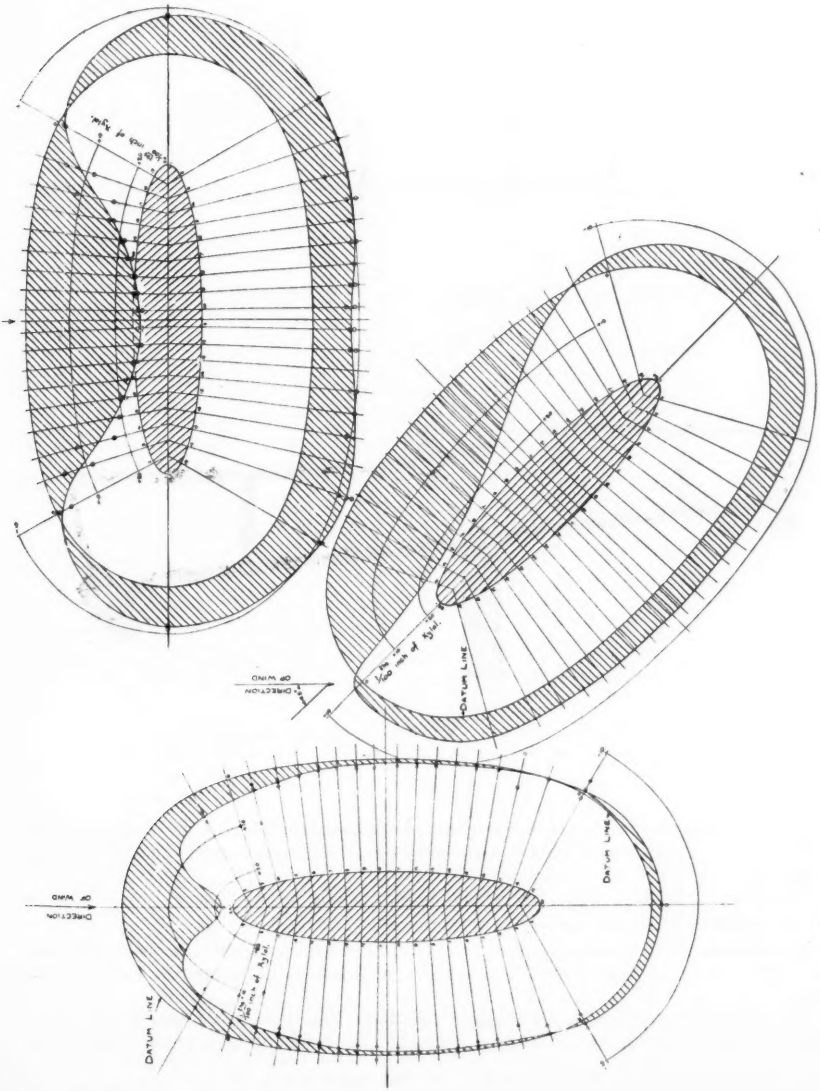



FIG. 12 FIG. 13 FIG. 11

DIAGRAMS SHOWING THE DISTRIBUTION OF PRESSURE AROUND AN ELLIPTICAL ROD

the longitudinal stability coefficient of various plan forms of aeroplanes. All the models are made interchangeable and the control planes are rotatably mounted in the link carrying the main plane.

Thus to obtain the control torque of any model, the position of the centre of pressure and the magnitude of the resultant lift is determined for various inclinations of the control plane, the main plane being kept at a constant predetermined angle. The series of readings may be repeated at other inclinations of the main plane to obtain the control coefficients at other inclinations of the machine. The restoring torque introduced by one degree variation of the inclination of the elevator, *i.e.*, the control coefficient may be readily determined from these readings.

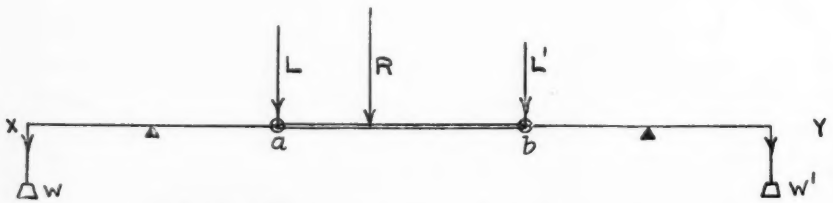



Fig. 14.—Diagram of Centre of Pressure Apparatus

Fig. 15.—Photograph of the Centre of Pressure Apparatus

Fig. 16 shows the front balance reading plotted against various inclinations of the elevator, the main plane being kept at a constant angle of 5 degrees, and the model having the plan form shown in the diagram.

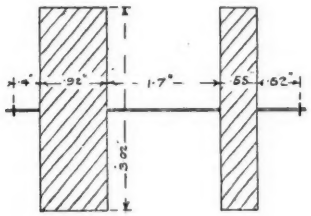
This diagram is given merely as an example, it being hoped to treat this subject in detail at a later date.

**Air Friction.** As a final example of the type of experiment which may be undertaken in the tunnel, we may take a brief account of a research to determine the value of skin friction.*

Although this research was not made in the tunnel described it is a suitable one for it. A sheet of thin glass, 36 inches long by 10 inches wide and $\frac{1}{8}$ inch thick (about) was

* *Engineering*, Jan. 24, 1913, pages 107–9.

suspended from a sensitive balance in the vertical arm of the wind tunnel and its total resistance measured at various velocities. A 2-inch strip was then cut off and the resistance again determined at various velocities. This was repeated until only a 2-inch strip was left of the original length. The resulting readings are shown plotted in Fig. 17. The resistance at a certain fixed velocity was then taken for the various lengths and was plotted against the length. (Fig. 18.) It was found that the resistance varied as the length within the errors of observation for all lengths of the glass above 8 inches. The resistance,



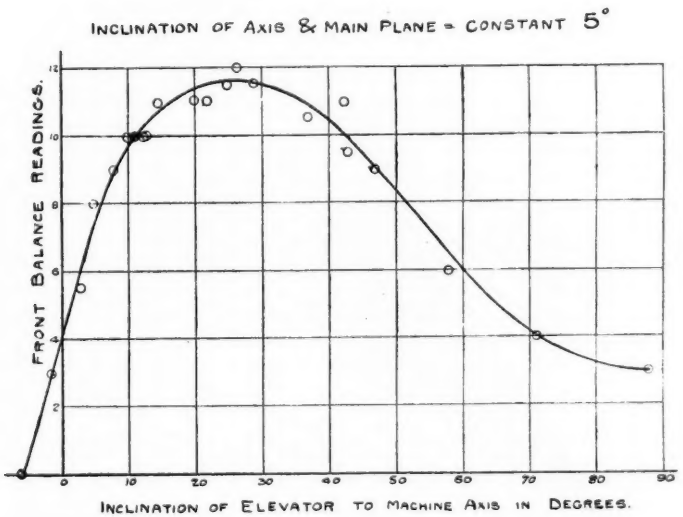




FIG. 16.—DIAGRAM ILLUSTRATING THE CONTROL TORQUE OF THE ELEVATOR

therefore, of the 8-inch length was subtracted from that of the 36-inch length and the results are shown plotted in Fig. 19. The equation of skin friction between 10 and 40 miles per hour deduced from this curve is :—

$R = \cdot0000098\ AV^2 + \cdot000317A.$

Where A = total area of surface in sq. ft.

,, R = resistance in lbs.

,, V = velocity in miles per hour, the air having a density of ·0012.

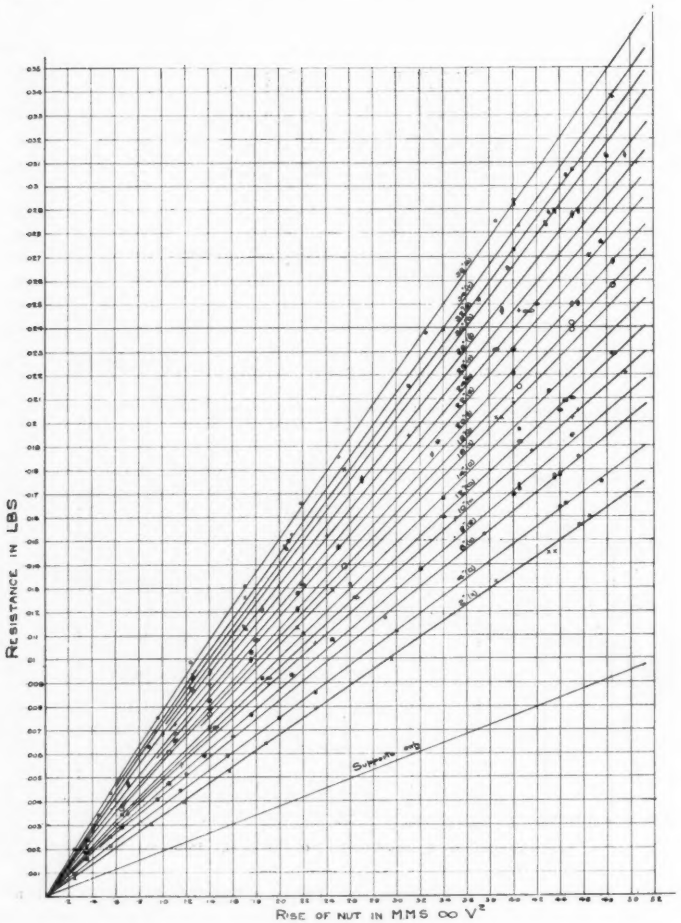



FIG. 17.—DIAGRAM SHOWING THE RESISTANCE OF VARIOUS LENGTHS OF GLASS PLATE IN "END ON" PRESENTATION AT VARIOUS VELOCITIES

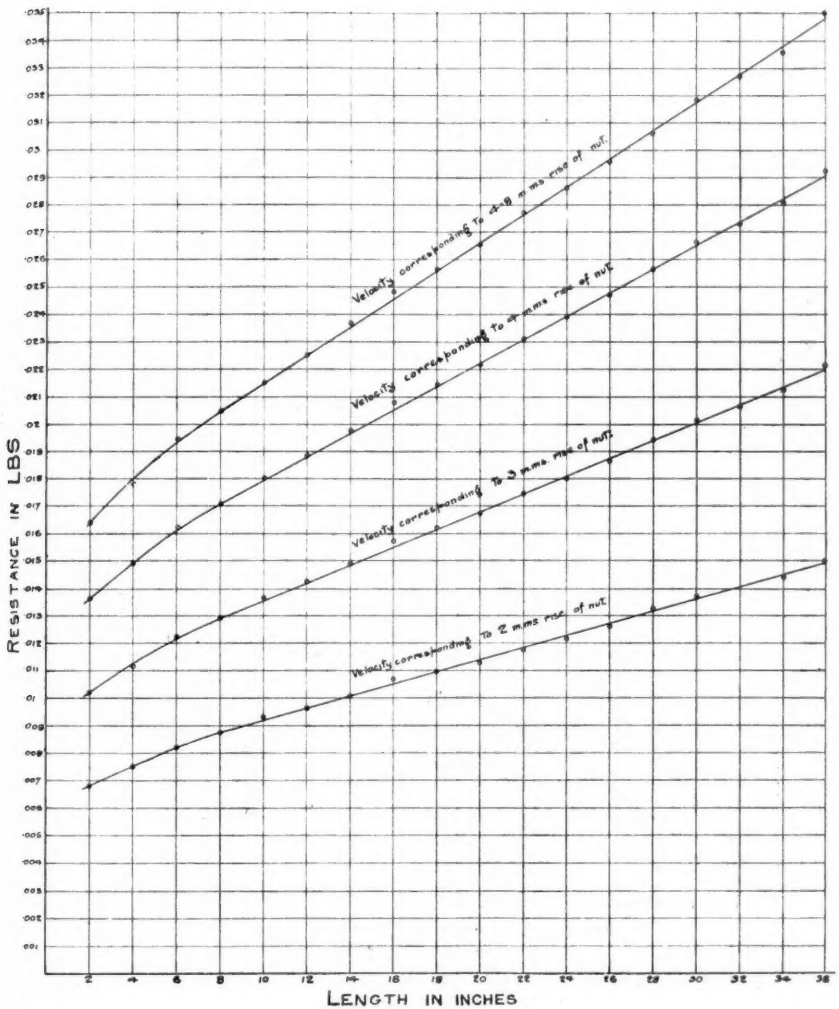

RESISTANCE IN LBS
LENGTH IN INCHES
Velocity corresponding to 4·8 m.m. rise of nut.
Velocity corresponding to 4 m.m. rise of nut.
Velocity corresponding to 3 m.m. rise of nut.
Velocity corresponding to 2 m.m. rise of nut.

FIG. 18.—DIAGRAM OF RESISTANCE AND LENGTH AT CONSTANT VELOCITY

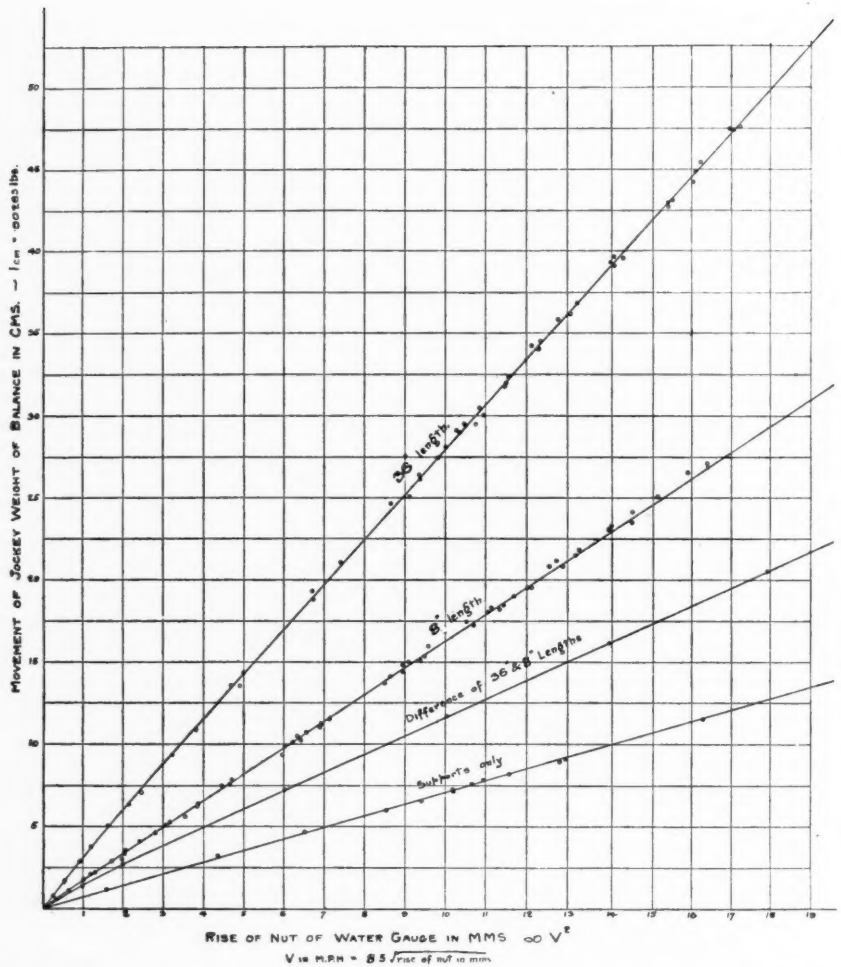

Movement of Jockey Weight of Balance in Cms. — 1 cm = 0.0163 lbs.
36" length
8" length
Difference of 36" & 8" Lengths
Subject to only
Rise of Nut of Water Gauge in MMS ∝ V²
V in M.P.H. = 8.5 √rise of nut in mms

FIG. 19.—RESISTANCE OF 36 IN. AND 8 IN. LENGTHS OF GLASS SHEET AT VARIOUS VELOCITIES UP TO 40 MILES PER HOUR

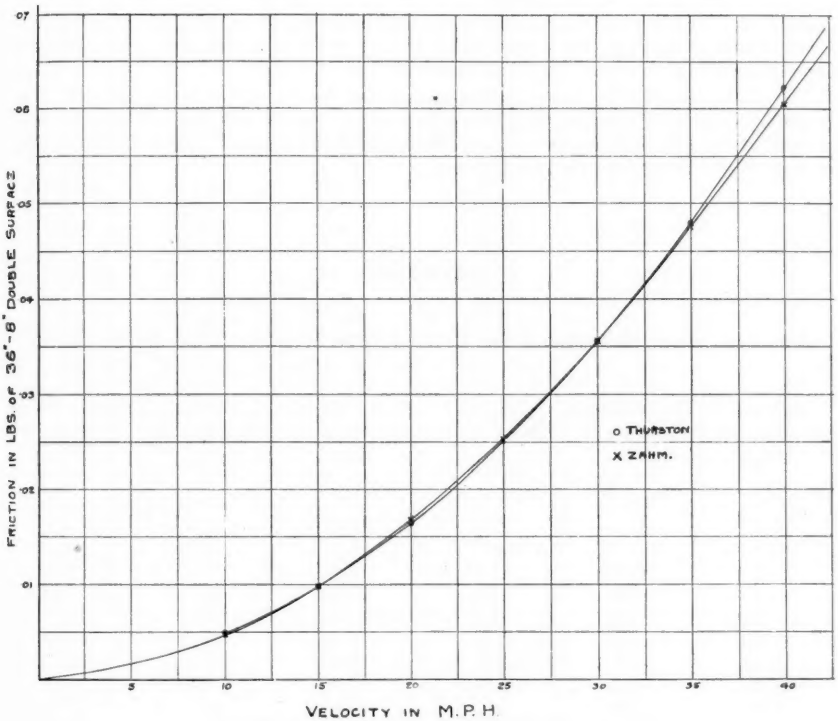



FIG. 20.—DIAGRAM OF SKIN FRICTION AND VELOCITY

The value of R must, of course, be doubled for all material surfaces, since such a surface has two sides.

The equation of resistance for a material plane having two sides therefore is :—

$$R = \cdot 0000196A\ (V + 32 \cdot 32)$$

the air having a density of ·0012.

This equation agrees closely with Zahm's equation for velocities up to nearly 40 miles an hour, as will be seen from Fig. 20. It gives higher values of the resistance for velocities above 40 miles per hour. The equation is in a form which is much more readily applied than Zahm's equation and therefore should be useful to engineers.

In concluding I should again like to urge the cause of research, because I am convinced that the extent of our advance and our position in the world of Aeronautics will depend very largely upon the amount of research work which we do. In this connection I cannot refrain from paying a very warm tribute to the splendid work which has been carried out at the National Physical Laboratory.

If we in this Society foster and encourage the great and increasing interest of our members in research work we shall be building upon a sure foundation and shall be doing a work which will most appeal to those who come in the future. Our reward, although we may have to wait for it, is sure to come.

## APPENDIX

SHEET OF GLASS 36 INCHES LONG BY 10 INCHES WIDE BY 0·135 INCHES THICK

| Balance Reading in cms. | Water Gauge Reading in mms. | Balance Reading in cms. | Water Gauge Reading in mms. | Balance Reading in cms. | Water Gauge Reading in mms. |
|---|---|---|---|---|---|
| 11·1 | 4 | 29·5 | 10·52 | 31·9 | 11·55 |
| 11·2 | 4 | 50·7 | 18·41 | 29·4 | 10·8 |
| 11·2 | 4·05 | 44·8 | 16·26 | 27·4 | 9·88 |
| 11·2 | 4·05 | 34·5 | 12·41 | 21·3 | 7·42 |
| ·75 | ·25 | 29·1 | 10·33 | 14·6 | 5·03 |
| 1·65 | ·55 | 27·1 | 9·54 | 39·3 | 14·14 |
| 2·85 | ·95 | 30·5 | 10·9 | 32·3 | 11·63 |
| 3·75 | 1·23 | 36·8 | 13·33 | 18·8 | 6·74 |
| 5·05 | 1·65 | 44·2 | 16·2 | 30 | 11 |
| 6·3 | 2·18 | 13·5 | 4·94 | 35·8 | 12·87 |
| 7·05 | 2·5 | 8·9 | 3·06 | 45·5 | 16·37 |
| 9·05 | 3·25 | 42·7 | 15·69 | 26·4 | 9·4 |
| 10·85 | 3·85 | 39·6 | 14·49 | 38·5 | 14·14 |
| 42·95 | 15·59 | 36·2 | 13·17 | 47·5 | 17·1 |
| 47·4 | 17·22 | 26·2 | 9·42 | 39·7 | 14·24 |
| 28·9 | 10·41 | 17 | 5·85 | 34 | 12·4 |
| 35·7 | 12·78 | 13·6 | 4·71 | | |
| 39·1 | 14·26 | 19·3 | 6·72 | | |
| 43·1 | 15·68 | 25·1 | 9·16 | | |
| 47·6 | 17·35 | 28·1 | 10·08 | | |
| 31·8 | 11·52 | 31·0 | 11·04 | | |
| 24·7 | 8·7 | 34·2 | 12·2 | | |

SHEET OF GLASS 8 INCHES LONG BY 10 INCHES WIDE BY 0·135 INCHES THICK

| Balance Reading in cms. | Water Gauge Reading in mms. | Balance Reading in cms. | Water Gauge Reading in mms. | Balance Reading in cms. | Water Gauge Reading in mms. |
|---|---|---|---|---|---|
| 6·45 | 4 | 9·8 | 6·05 | 15·9 | 9·6 |
| 6·55 | 4·05 | 11·5 | 7·15 | 14·3 | 8·98 |
| 6·6 | 4·05 | 18·4 | 11·46 | 9·3 | 5·98 |
| 6·6 | 4·05 | 20·8 | 12·95 | 4·6 | 2·83 |
| ·6 | ·35 | 23·3 | 14·15 | 11 | 6·9 |
| 1·1 | ·65 | 27·1 | 16·53 | 18·3 | 11·15 |
| 1·8 | 1·05 | 15·1 | 9·42 | 20·8 | 12·62 |
| 2·25 | 1·35 | 10·2 | 6·43 | 23·1 | 14·07 |
| 2·9 | 1·75 | 3 | 1·86 | 25·1 | 15·3 |
| 3·55 | 2·1 | 7·8 | 4·74 | 21·2 | 12·8 |
| 4·1 | 2·45 | 10·4 | 6·36 | 14·8 | 8·97 |
| 5·25 | 3·2 | 13·7 | 8·52 | 11·2 | 6·94 |
| 7·4 | 4·5 | 18·2 | 11·07 | 10·1 | 6·23 |
| 21·5 | 13·27 | 19·5 | 12·08 | 5·1 | 3·1 |
| 19·5 | 12·19 | 21·8 | 13·34 | 3·4 | 2·09 |
| 19 | 11·73 | 17·2 | 10·73 | 2·1 | 1·23 |
| 17·4 | 10·56 | 15 | 9·12 | 5·6 | 3·59 |
| 15·8 | 9·98 | 6·2 | 3·89 | 23·5 | 14·66 |
| 14·4 | 9·12 | 26·5 | 16·03 | 24·2 | 14·68 |
| 19·6 | 12·21 | 18·2 | 11·38 | | |
| | | | | 7·6 | 4·69 |
| | | | | 10·7 | 6·57 |
| | | | | 14·1 | 8·67 |
| | | | | 15·3 | 9·51 |
| | | | | 18·1 | 11·1 |
| | | | | 21·6 | 13·28 |

## DISCUSSION

Col. FULLERTON, R.E.: I should like to congratulate Mr. Thurston on his very interesting and useful lecture. Papers of this kind are most helpful; I hope members of the Society will take this subject up and try experiments on their own account. No expensive apparatus is required, and provided the work is carefully done, very good results should be obtained. As regards the method used in plotting the diagrams (see Fig. 12), I think the plan I followed when making experiments of this kind some five years ago is perhaps better; it is shown in Fig. 12a.

The half length of the oval is developed, and the pressures, positive or negative, set up along the line AB, the ends of the ordinates forming the curve of pressures. If this pressure curve area is divided, so that the part in front of DE is equal to the part in rear of it, then F is the centre of pressure of the surface, and if (see plan Fig. 12) corresponding points F are joined by a line which cuts the centre line of the strut, the point of intersection is the centre of pressure of the strut itself.

The form of the pressure curve is interesting; it will be seen that all the high pressure is at the front end of the strut, the fall of pressure at the sides being very small.

This seems to point to two things: first, that the coefficient of skin friction must be small; and second, that a great reduction in total resistance would be obtained if

a prow (and possibly a stern) was added to the oval, making the section more like the body of a bird with outstretched neck. I suggest that experiments to test this latter theory might be carried out, as they would not entail much further expense.

Mr. F. H. Bramwell: I would like to ask Mr. Thurston if he can give us any information as to the value of the maximum positive pressure on the front of the various bodies he has tested. At the National Physical Laboratory we have invariably found that the maximum positive pressure on the front of any body in a current of air is equal to the pressure in the dynamic tube of the Pitot tube.

Mr. Thurston has shown us an ingeniously simple balance for determining the magnitude of the lift on a model aeroplane and simultaneously the point where the resultant force on the aeroplane cuts the line joining the two points of support. I venture to suggest that Mr. Thurston should also determine the drift on these models independently, so that the measurements may be more general; as then the resultant force is completely determined in magnitude, direction and position, so that the moment about any point in the aeroplane (*e.g.*, the centre of gravity) can be determined.

Mr. F. Handley Page: I have read with great interest Mr. Thurston's paper, and must congratulate him most heartily on the results he has obtained with such a simple apparatus. I would like to ask him what precautions, if any, he took to insure

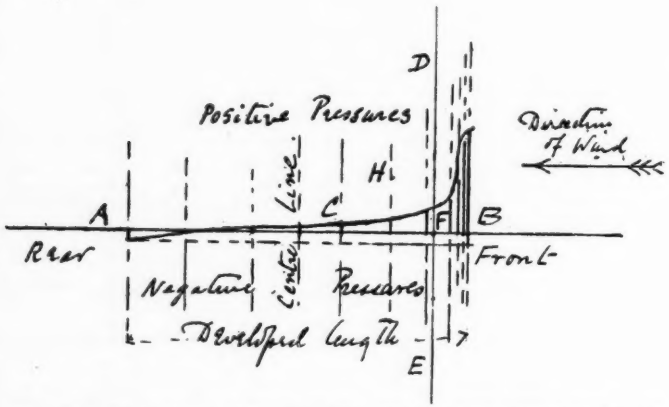



Fig. 12A.—Alternative Method of Plotting Results shown in Fig. 12

that the current in his tunnel was uniform, and whether the final results obtained before the experimental researches were started were such that there was no possibility of error from uneven air-currents. Nearly all the experimenters in different countries have met with trouble on this score, and in the latest *Bulletin de L'Institut Aerodynamique de Koutchino* Riabouchinsky states that all the previous results must be modified by 3 per cent. in the case of pressures measured owing to the velocity of the air being less than what he had previously taken. Perhaps Mr. Thurston will advise us as to the steps he took to avoid any trouble in this direction.

Curves of air pressure distribution are given in the paper for rods and bars of different dimensions. Has Mr. Thurston taken any others on planes of cross-sections similar to those in use in ordinary aeroplanes? These last are most instructive, as they give us a picture of what is actually going on round the plane.

In the experiments of Drs. Finzi and Soldati the air pressure distribution on cylinders or spheres always had one similarity, no matter what was their size. If tangents were drawn to the surface at the point of zero pressure, the angle between them at their point of intersection was invariably about 43°. I would like to ask Mr. Thurston if he has met with any similar occurrences in his results.

Finally, I would like heartily to endorse what Mr. Thurston has said as to the necessity for further aeronautical research, and I trust that as the result of his simple and clear exposition of how cheap and efficient aeronautical research apparatus can be made that there will be a great increase in the amount of research done. The title of Mr. Thurston's paper can then be altered to "Aeronautical Research in *Every* Home."

Mr. THURSTON, in replying to the discussion, heartily thanked the Chairman and the various speakers for their kind remarks and the meeting in general for the kindly reception they had given to his paper.

In replying to Col. Fullerton he stated that he had heard with interest of his experiments, and agreed with his remarks with regard to the method of setting the results out. As a matter of fact the method referred to by Col. Fullerton was adopted when the readings were first plotted, but in order to give a graphic eye picture to illustrate the lecture the radial method was used. Both methods have advantages and disadvantages, and preferably both should be employed. The research proposed by Col. Fullerton with regard to the effect of a prow and stern would be interesting, and it was to be hoped that someone in the meeting would undertake the experiments. In reply to Mr. Bramwell the lecturer stated that, as at the National Physical Laboratory, the maximum positive pressure on the normal portion of the surface facing the wind was found to be approximately equal to the pressure in the dynamic tube of the Pitot tube. The lecturer fully agreed with Mr. Bramwell's suggestions with regard to the methods to be followed in using the centre of pressure apparatus.

Mr. Handley Page in his interesting remarks had touched upon one of the most difficult problems in wind tunnel work, namely, that of obtaining a uniform air-flow. It was almost impossible to obtain a perfectly uniform flow. However, by carefully exploring the velocity over the section to be used, and by a suitable arrangement or adjustment of baffle plates and inlet and exit mouths, it was possible to get a fairly even flow over a certain section. The point at which to place the Pitot tube to obtain the truest velocity reading was also found. The lecturer stated that he had not made an investigation of the pressure distribution around cross-sections similar to those in use on ordinary aeroplanes or around cylinders or spheres of various diameters, and he thought that this would obviously be good work for anyone willing to undertake the experiments. The angle subtended by the tangents at the points of zero pressure in Fig. 4 was approximately the same as that found by Drs. Finzi and Soldati, and there appeared to be no reason for assuming that the angle would vary with the diameter.

The CHAIRMAN, in proposing a very hearty vote of thanks to the lecturer for his admirable lecture and clear diagrams, said that the paper fell into two parts, that dealing with the technical results of research and that dealing generally with the question of carrying out research. He was in full sympathy with the views of the lecturer on the latter point. He could think nothing more fascinating to a young man with scientific tendencies than to take his part in exploring the *terra incognita* of aeronautics. We had all felt the spirit of investigation within us. Any one of us might obtain results of incalculable value to the conquest of the air, which had only just commenced, and the carrying on of which mainly rested with the younger generation. To help this work the Council of the Society had been considering the organising of the students' section of the Society much on the lines of the similar section in the Institution of Civil Engineers, and he hoped that before long something might be done in this direction. He thanked the lecturer very much for his valuable and suggestive lecture.

The vote was carried with applause and the meeting terminated.

THE NINTH MEETING of the Forty-eighth Session of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on Wednesday, 26th March, 1913, at 8.30 p.m. Major-General R. M. Ruck, C.B., Chairman of Council, presided.

DISCUSSION ON

## THE EFFECT OF THE GOVERNMENT AERONAUTICAL PROGRAMME UPON THE TECHNICAL INDUSTRY

The CHAIRMAN : I think most of you are aware that we were to have had a lecture to-night by Captain Samson on hydro-aeroplanes. The Society very much regret that that lecture could not be delivered, but the Admiralty have intimated that they thought, after careful consideration, it was not in the interests of the service that the lecture should be delivered. Therefore we had to decide what to do to-day, we had not much time, and as we could not get another lecturer forward we thought it would be useful to various members of the Society, and possibly to others outside the society, if we had a discussion on this subject. We have a large number of members in our Society associated with the technical industry. I am in the unfortunate position of being told off to open this discussion, which I propose to do with a few general remarks. Without wishing in any way to limit the discussion, I may venture to suggest the most important matters which might possibly appear to be within the scope of the discussion. The Government programme may, I think, for the purpose of the discussion, be divided into three categories, that connected with aeroplanes, that connected with aeroplane engines, and the third category connected with airships. Under the first, with regard to the aeroplanes, the question arises: are manufacturers prepared to build machines in accordance with the War Office specification, or must they still require to continue with their own designs? In either case can they complete aeroplanes at the rate indicated by the Secretary of State for War? I think he spoke of a few weeks to build aeroplanes. Then another question that would seem to arise is this: are manufacturers prepared to build the fighting type of aeroplanes, a much more powerful, stronger, heavily engined type. If they are, as this is a new departure too, it will be necessary to many manufacturers, I take it, to have a certain number of orders, and it may be necessary also that they should have to make various trials and experiments which will cost a good deal of money. Then we come to engines and the question arises there, does the trade generally approve of the system of one prize and a large order, or would they prefer a more extended distribution of prize money and orders. Any remarks also on the types of engines which might be specified in a Government programme might be useful this evening. As regards airships, I am sorry to say that we have not had the benefit of Mr. Winston Churchill's speech on the Naval Estimates, and therefore we do not quite know what is the Government programme. I think it might be interesting if anybody would speak on the subject, if there are manufacturers here who are prepared to commence building—I believe one or two have commenced building, I am told so—large airships. If so, and if they are about to start, one would like to know what conditions would be acceptable to the manufacturers, I do not mean the exact amount of money, but what is the nature of the contract, whether the builder would think it necessary to disclaim responsibility as regards the result and whether he would undertake to work on a lump sum contract, or whether he would work on a percentage contract. Finally and generally, is the number of aeroplanes contemplated by the Government sufficient to keep alive the existing industry? If not, what numbers would be suggested as adequate? Then there is one other point I think we might touch upon and that is, can the industry assist the Government in any way in training personnel, and keeping them available in the time of war. That is all that I suggest for the moment for opening the discussion.

Mr. HOWARD FLANDERS : It will be a good thing if we first have a clear understanding as to what the Government policy really is. For the Naval policy I cannot speak, as I know nothing about the lines on which the Navy will work, but in my opinion, the new Air Department at the Admiralty has done more in two months than the War Office has done in a year. The Army policy, as outlined by Colonel Seely, is clear. In his statement on the Army Estimates on Wednesday last, after speaking of the many virtues of the machines built by the Royal Aircraft Factory, machines which I admit combine many of the excellent points of other machines, he said : " I have indicated that we have in our judgment the best aeroplane in the world, which we ourselves have devised, and can manufacture in any number that we please." That at least is something definite.

In considering the effect of Colonel Seely's recent policy on the aeroplane trade, more especially from the standpoint of the small constructor, let us first view the commencement of the present phase of practical aeronautics in this country.

The names which stand out first are Messrs. A. V. Roe, Cody, Barnwell, and Willows. They persevered and faced privation and abuse, not necessarily with a view to ultimate gain, but largely owing to the fascination with which the science of aeronautics holds all those who start practical work.

Mr. A. V. Roe and Mr. Cody have shown that it is possible to succeed in England, though, of course, slowly. Had these gentlemen been encouraged and assisted from the commencement of their experiments, England, instead of being behind all civilised nations, would have been far ahead. For although it is possible by the aid of rhetoric to make us appear efficient, yet really we are far behind and are in a chaotic and disorganised state.

In these early days the struggle was a very hard one. I, personally, know one constructor who was spending all his small income on experimental work and living on less than 12s. 6d. per week.

Now, I had the good fortune to start under the instruction of Mr. A. V. Roe, and, shortly after leaving him, sank all my very small capital in experimental work on monoplanes. I was fortunately rewarded by some success. Then, having produced a machine, the next thing was to know what to do with it.

I looked around and found that no one wanted aeroplanes in this country, and I was thinking of going abroad. Then Colonel Seely came forward and said that aeroplanes would be required by the Army. His speech in this building will be long remembered by those who were present. Things seemed bright and hopeful ; and I induced some friends to give me the financial assistance necessary to undertake the order with which I was favoured. I had troubles in complying with one of the tests, but that was overcome satisfactorily.

On inquiring if further orders were likely to be placed I was told that we must wait and see how our machines behaved in use. Also that the monoplanes would have to be altered to comply with the report of the committee which was considering the safety of monoplanes.

Now this Committee met and considered the subject and drew up a report with which I am in entire agreement, but although the report was finished and dated on December 3rd, 1912, and I repeatedly asked for a copy, no copies were issued till February 4th. I received mine on the 7th, two months after the report was dated. After a little correspondence to settle methods of construction I quoted for the required alteration on February 12th ; since then I have heard nothing, although that is six weeks ago.

So far this year the only information that I have been able to obtain is that I shall not be called on to tender for B.E. machines, hence I have been compelled, much against my will, to discharge my trained mechanics, who in disgust have abandoned the aeroplane trade. These men were trained with difficulty and at considerable expense.

Now Colonel Seely has stated that he will have by May 31st 23 more aeroplanes than he requires.

I doubt very much if one-half of these aeroplanes will be capable of flying efficiently when required, but that is none of my business. As far as I am aware the total number

of efficient aeroplanes—and those but slow machines—which were in such a condition as to be ready for immediate safe use on last Monday was but 38 (17 at Upavon, 2 Maurice Farmans at Lark Hill, 14 at Farnborough, 5 at Montrose). These are all in the Royal Flying Corps sheds. The 30 machines in the Factory sheds cannot be considered in immediate flying order. Moreover, there are not more than about 48 British machines on order.

The serious aspect of the statement by Colonel Seely is that, according to him, he has 23 more machines than he requires.

Does this mean that there will be no more orders placed this year? I fear that it does. The excuse which will be tendered is doubtless the so-called inability to obtain aeroplanes. Now what is this due to? The first and after all the only reason is that the *aeroplanes were not ordered.*

No aeroplanes were ordered by the Army from British makers between September, 1912, and January, 1913.

Now, the effect of delays in obtaining orders on small constructors is fatal. Their standing charges go on, for they must be ready if and when a large order comes to be rushed through. No firm with a small capital can stand this sort of thing long.

In connection with R.A.F. specifications I have been caused annoyance by hypercritical inspectors and illegible drawings. For instance, "$3\frac{1}{8}''+2$ thicknesses of 12 S.W.G. nearly," cannot be called an intelligent or accurate method of dimensioning drawings, which are sprinkled with the Greek alphabet to mystify the British working man.

We were assured only a year ago that the Royal Aircraft Factory was chiefly to provide us with data and to conduct experiments that we could not afford to do ourselves.

Sir, I ask you how much of the data which are obtained are published in the Blue Book, or can be obtained even under a pledge of confidence, by an independent constructor?

Not only is what has happened the direct opposite to the promises of a year ago, but it is also a cruel thing to those constructors who are not wealthy, because had we known at the commencement what we know now, many of us would have left England for certain foreign countries, which were anxious to get constructors a year or so ago, or perhaps we would have cut our losses and abandoned the science.

Is it not strange that a country of this importance and wealth can do nothing for those who have spent all their time and money in studying the science of aeronautics?

Lest you should think I speak from the standpoint of one who has failed in his work, perhaps I may be permitted briefly to compare my monoplanes with the B.E. of the same date:—

| | | |
|---|---|---|
| Slowest Speed Monoplane | 41 | |
| B.E. | 42 | |
| Fastest Speed Monoplane | 67 | |
| B.E. | 72 | |
| Climbing Monoplane | 330 | ft. p. min. |
| B.E. | 450 | „ „ |

Factor of safety on the monoplane considerably higher. We had to use the same propeller for all tests. The B.E. 2 used two or more propellers, giving it a great advantage.

The monoplane can be flown without exertion on the part of the pilot in high winds of 10 or 15 miles per hour faster than the B.E. 2 even though the biplane pilot had to work hard.

At the last Paris Aero Exhibition more than three-quarters of the machines which were the property of the French Army were superior to any machines which our Army have in actual use at the present time, while to compare our dirigibles with those in use in foreign countries is a joke.

If the start of the *Daily Mail* Race be recalled it will be remembered how vastly superior all monoplanes were to any biplane.

The new 100 R.A.F. biplane achieves a speed of 91 m.p.h., a proof of colossal inefficiency. Several French aeroplanes of like power being 10 m.p.h. faster, I fear that this biplane, even if it was flown by the "brave young man," would be of no account against many foreign aeroplanes of the same power. Incidentally, an 80 h.p. Wright biplane with a water-cooled engine has exceeded 100 m.p.h. having done a complete lap of a small course at 99 m.p.h.

Sir, it is unnecessary to continue. In the face of this official attitude is it surprising that managing directors of large engineering works, and men of wealth receive the proposals of aeroplane constructors with derision? But, Sir, we have always the workhouse.

Colonel CODY: I will say a few words about this question of supplying the starving aeroplane manufacturer with work. I disagree with one or two items of Mr. Flanders' remarks. Of course I am a very jealous constructor myself and that is why I disagree with him. I might say if anybody has had cause to grumble I have the first cause, because I built the first aeroplane that flew in England and I built the first man-lifting kite that flew which led the way for the aeroplane abroad, and I put wings on the kite which I built before the Boer War started. I think all that is before anything, except perhaps Hiram Maxim's propeller which lifts straight up. There was nothing I copied that I had not done before other people did it. It is hardly copying, because I put it into a flying machine from a kite and into the kite from the bird. Therefore I compare my machine which was built under the auspices of the War Office, entirely my design—I believe they will bear me out in this, in fact it was presented to me because I had divulged my secrets in it, and they did not wish to keep my secrets behind after I left, so they presented me with the aeroplane, and they were of opinion at that time that it was of no use, I firmly believe, or it is a very strange change of policy that caused them to give away good property. I was quite sanguine about the capabilities of my machine if I could possibly get a 100 horse-power engine, and the gentleman who was my boss at the time I built the machine is in the room to-night and knows that I begged for a 100 horse-power machine. When I started the machine, and before I built it, it was my prayer always to get something upwards of 100 horse-power for my machine. Eventually I got an engine which gave 8 horse-power more than I built my machine to carry, and I would like to compare my efforts, with a paltry £3,000 to spend, against the Aircraft Factory with something like £150,000 or more. I was challenged by the Aircraft Factory's machine or by the driver of it, and I laughed at him and told him that he would not win the race. He was offended because I spoke to him in the manner I did. However, it came to a race and the gentleman who started the machines is not in the room, but the gentleman who saw the finish of the race is in the room, and I think he can vouch that I came in at least 200 or 300 yards in front in a mile and a half. Of course I could not continue improving my machine on account of the lack of money with which I had to work. I had not a shed with a floor in it, so I had to work in the muddy ground, and it is a swampy place where my shed is. So that I think Mr. Flanders is wrong in speaking of the superiority of the French machine over the English because my machine remains almost in its same old condition, and when it went to Salisbury it met all the French machines that were willing to come to Salisbury, and I think it met them on fair ground and it proved its superiority as a weapon of war and as a machine built for war purposes. I was a servant of the Government, building for war purposes when I built it originally, and I continued three years and allowed the world to go on against me without altering my design, and I still came up and beat them after all that time. That only shows you when I first started I was three years ahead of the others; it proves it, there is no question about it. I got yesterday my first propeller built to my own design. This propeller was built by Chauvière, and it is my ideal which I impressed upon the balloon factory so severely and so hard, and I have it now in wood, not in metal, and I hope to try it very soon. With regard to the question of French machines I would be prepared to bet £1,000 within the next—any time—if they would bring a machine over that was built before the military trials came off and compete against me. I am willing to lose £1,000

of the money I won this summer if they beat me in the conditions of the military trials. I made the propeller that was on my machine in the military trials and I think if I had had a propeller made by a good firm, who understand propeller making, that my machine would have done even better than it did in the military trials. I should like to have some more orders for aeroplanes, I have only had one and I think I have shown a great deal of patience. I have tried very hard to get more both from the Army and the Navy. I have never sold an aeroplane to any other Government yet—perhaps it is because I could not! I have not tried hard either. I think really if this Government do order aeroplanes or encourage anyone to build aeroplanes, they might give me a look in. I have on order now an engine of 500 horse-power which I hope to put in an aeroplane, and I hope if I cannot get the Government to order this aeroplane that I can go round with my hat and collect the money from the public, and then I will present the aeroplane to the Government when I have finished. That is going to be my system; I am going to give lectures in the provinces and try to collect the money to build this big aeroplane, and it will be an aeroplane after my heart's desire, what I have wanted for a long time, something big, something that will carry two guns to fight and I hope it will fight well. It will need very little work to drive it, I am sure of that. I wish you to take that point very seriously that I am still open to compete against English, French, American or any aeroplane that can be produced here and to be adjudged by any committee who wish to appoint themselves. What I did at Salisbury may not be all I can do. I am sure I can climb more. I think if the Government are going to insist on a B.E. type machine and make constructors build the type that they choose, it will be a mistake. Some parts of it I have made before and can make again, but there are other parts of which I do not approve, and it would go against the grain if I had to manufacture machines of which I do not approve. I do not want to make two machines alike really. My hopes are in experimental work. If the Government would choose their aeroplane and take it out and put down a written condition, and let the aeroplane go through the evolutions and performance of these conditions, and say: "Now construct it; if you can supply an aeroplane that will do that we will buy it," that would give the constructors a fair chance. If their aeroplane is superior to anything in the world they would easily have a chance of proving that, because their aeroplane would do it. They would not be asking you for impossibilities. They would say: "We ask you to build and do what we can do." That would be fair and I would be prepared to build to-day what they do. I might be a little heavier, but if I was heavier that does not matter to them as long as I execute the performance, as long as I fly and carry the same weight at the same speed, climb, and land in just as rough ground, and so forth. If they would put those conditions out to the constructors instead of stipulating that they must be the B.E. type it would be better. I am sure if I were an officer fighting I would condemn it immediately because of its screw. I cannot agree to a machine with a screw in front. I built a monoplane with a screw in front and I did not like it, it hit a cow! Still there are advantages to a screw in front or rather an engine in front. That is my experience as an aeroplane pilot.

Captain Wood: They say one should not look a gift horse in the mouth, so that I suppose it is equally objectionable for us to criticise in any way Government methods. I do not propose to do so. The Government policy in the Army Estimates seems from the aeroplane constructors' point of view to be summed up in the fact that £234,000 has been allocated this year instead of £100,000 which was allocated last year. There is no other mention in the Estimates of any sum being put aside for the purchase of aeroplanes. Therefore presumably aeroplanes are to be purchased out of this sum. That rather brings me to the question of how much will be needed by the Factory for experiments and how much will be allocated for the purchase of aeroplanes. There are rumours which one has heard of an experimental dirigible which sounds an expensive thing. One hopes it will not be too expensive, and also that it may be untrue, because one can imagine a large amount of money might be spent upon this machine which we aeroplane constructors would like to see spent otherwise. What principally interests us at the present moment is how much of that £234,000 is going to be diverted into our pockets. To obtain some of that money I take it we have to produce a machine that is acceptable to the Government. That will be paid for by the Finance Department and will have to be passed by the Royal Army Aircraft Factory. To do that it would be much easier

for us as constructors if the Factory or the Government, or whoever is acting on their behalf, would, as Mr. Cody said just now, lay down tests and state that they would be prepared to accept any machine that would pass these tests. They could specify whether it was a question of an engine in front or behind, that is a matter of detail.

They might specify what test they want and say whether they are prepared to buy a certain number of machines which could pass those tests. If they did that designers would be able to start out with some idea what they were trying to design a machine for. Now a good many designers are designing machines to be what they think is the best machine flying. That is a most laudable aim and object, but if it does not fall in with the Government's view it is not a very profitable one. If we could get a standard, designers would know what they had to meet and we should know what we were up against, so to speak. The Army trials last year set us a standard, and I think I am right in saying that very few machines met with it. I do not know if the same standard is still what is considered to be required by the Government and the Aircraft Factory. But firstly I would suggest that the 4½ hours fuel capacity might be reduced on a certain type of machine, on the machine of a certain type which will not be required for long reconnaissance, and it will be easier in that machine to reduce the necessary load it has to lift provided you demand other requirements for it as one imagines the Government would. Then another thing which would assist us from the aeroplane constructors' point of view would be if the Government would say how many machines they want of a certain class. We have no knowledge of how many machines the Government propose to buy. In the Naval Estimates in any year it is definitely stated in Parliament how many battleships, &c., they are proposing to buy and how many they propose to lay down, but we have no knowledge of that. All we know is that a certain sum is to be allocated to the Factory and how much is to go in aeroplanes it is impossible for us to know. All we know is that double the sum this year is being allocated as compared to last year. Therefore perhaps we may hope to see double the amount of machines bought. Then there is one other point I should like to refer to with regard to the Aircraft Factory. The Aircraft Factory, we are told, exists for the sake of experiments and for experimental purposes. Cannot these experiments be published monthly? It would assist a large number of constructors in this country and save a vast amount of money. At the same time one sees that it may not be advisable to publish a certain amount of information in the papers where anybody could get it, but in answer to that it would be a simple matter to put a certain number of aeroplane firms on the Government list, and to them, perhaps, these experiments, which without doubt are valuable and which without doubt are more searching than any carried out, or known to be carried out in the world up to now, could be published and circulated, say to a certain number of aeroplane constructors. If that were done, without doubt the aeroplane industry would get on quicker in this country. I take it that is what the Government wish and the Aircraft Factory themselves wish.

As regards motors they made a statement in the House the other day that a four-figure prize would be given for motors. And no doubt shortly we shall hear what type, kind, and horse-power motor is required. The sooner we hear it the better and the quicker we shall be able to meet it. You, sir, in your opening remarks, said that Colonel Seely stated that the Government would require manufacturers to be able to turn out machines in a few weeks. There is no difficulty in doing that provided we are given a certain amount of notice, but what is at the present moment very hard and almost an impossibility is to suddenly have orders sprung on you and then be asked to turn them out in a few weeks. The materials for the construction of aeroplanes are not easily obtained; they are still rare, and the Aircraft Factory especially know this probably better than we do. The materials are rather high grade, high and special stuff, and many of them have to be manufactured especially for the particular purpose. These materials also are changing. The requirements of each individual lot of aeroplanes change and the material wanted for one is not the same as the material wanted for another, and that does lead tremendously to delays. It is impossible for manufacturers to undertake delivery in a few weeks when we cannot get our material from the wholesale manufacturers of the material.

Mr. JOYNSON-HICKS: I wanted to-night to obtain further information of the views of the manufacturers and designers upon the effect which the Government programme will have upon the aeroplane industry. Two things I think are certain, and those who are designers and manufacturers must realise one of them. I want to do all honour to the Government, and the first thing the Government has to consider is clearly not the interests of the trade but the interests of the nation. I think that must be accepted by everybody. They have in the first place to do what is best for the Army and Navy, and the trade comes second. That I am prepared to admit. Then the question does seem to me to come whether they can do what is desirable in the interests of the nation without considering the effect upon the trade, because after all it seems to me that we must in time of difficulty, certainly in time of war, depend for our aircraft upon our English manufacturers. I agree that it is desirable to have the best of all foreign machines, but in time of stress, when we are engaged in naval warfare, we could not get without very grave difficulty, probably not get at all, aircraft either from France or any country, and we must depend upon our English manufacturers. I strongly agree with what I have heard to-night, and what I have tried always to advocate is that the only way to produce an adequate industry in this country is a steady persistent flow of Government orders. There are, perhaps unfortunately, no ordinary private demands for aeroplanes at the present time; practically none. The only outlet for the manufacturers on any large scale is the Government, and the question which I think is of vital importance, not merely for the nation but for the manufacturers, and incidentally the nation, is whether the Government are taking the best course in order to develop the industry in this country. We are slow, we have been undoubtedly in this country in getting on with this industry. It was not quite recently so highly developed as it has been in France, and equally in France there has been very little demand from the ordinary private members of the public for aeroplanes. The great bulk of the aeroplanes has been for Government service in France or the service of other Governments. That has ensured many of the French manufacturers a steady supply of orders, and if we are to hold our own with foreign manufacturers we must have a very strong and steady supply of Government orders. Spasmodic orders, if I may be allowed to say so, can be no real use to any industry, particularly to the workmen. I know the difficulty from the commercial point of view of keeping works going with spasmodic orders. You cannot get your best men (by men I mean the mechanics) unless you have a steady supply of work. Therefore spasmodic orders tend to bad workmen, and it is only by the flow of steady orders that we can get what we want. I should like the designers to consider—I may be talking heresy here—some vital variations in the designing of hydro-aeroplanes. I look forward to the time when the Navy will have hydro-aeroplanes of a very much more powerful character than anything that has been made at the present time. I know with land aeroplanes that the policy has been at present to have light and powerful machines up to the extent of 100 or 110 horse-power. I may be a visionary, but I see no reason why the hydro-aeroplane, at all events, should not run up to 1,000 horse-power or even higher, and it seems to me that the development of these machines in the future will be in the form first of very light small monoplanes, not so large, not any larger in their body than a torpedo, which may be shot off the deck of a vessel, and, on the other hand, a series of very large and powerful hydro-aeroplanes all round our coasts for coast defence purposes. I know I am talking in the presence of Mr. O'Gorman, who is the head of the experimental side of the Army, and naturally his views will largely affect the policy of the Navy too. I cannot help thinking that is a direction in which we may develop, which has not been developed by either France or Germany, and that if we can get in front of the other countries with a machine of that kind we shall be able to make up some of the leeway which at present undoubtedly exists in regard to both airships and aeroplanes. I came here with a view of learning and ascertaining the desire of the trade and the designers; I am not here as a politician, but as one interested in aeroplanes, and if you can give me any information and suggestions which will enable me to be of use to the industry for which I hope to see a very great future I shall be only too pleased to use them as far as I possibly can.

Mr. BERRIMAN: I did not come here to talk and I am not a constructor. I wanted to hear what the constructors had to say. I do think, however, over this matter that

we are a little bit apt sometimes to kick the wrong football, and the common football to kick in this country is the Army. I am not sure whether we ought not to look more closely to the doings of the Navy. The Navy we are all afraid of, and do not like to inquire too closely, and we take care we do not. The truth of the matter is, that is the unspecified factor in the Government programme, and the factor that lends itself to development at the present time. As regards the Army, so far as Colonel Seely has specified it, he has laid it down clearly, and nobody can complain of what he intends to do. He intends to provide an air force suitable to accompany an expeditionary force, and that force is to consist of eight squadrons. Therefore you have it, and the only thing I complain of is that he does not bring the air force up to its establishment within a reasonable amount of time. It seems to me that he might bring it up quicker. You cannot bring anything of that nature up to its establishment without spending money on items of capital expenditure, and that is a point that has not been sufficiently criticised in Parliament. Sums are lumped together in a curious sort of way, and a quarter of a million is put down against the Royal Aircraft Factory, and I only hope that Mr. O'Gorman will get anything like that amount to experiment with. People are accusing him of being likely to complain because you cannot get transport and barracks and machines out of that sum. We had a meeting at the Navy League the other day, and the figures have been published—I think I have them right—that Colonel Seely has arranged for five squadrons of aeroplanes to be brought up to their establishment this year. I do not see why he should not have brought the entire military wing up to its establishment this year. It would have been reasonable, I should have thought, and they might have done so if they had spent the money. That will cost at least £120,000 in machines, and the transport of that will cost another £120,000. I may be wrong to several thousand pounds, but there are the general facts, and you can work out how much transport is likely to cost when every aeroplane has to have a transport motor. It is an expeditionary force and has to be carted about. It is not appreciated how much money has got to be spent on transport, and then there is that flying workshop, whatever you call it, which does not seem to have materialised in any way. You have to establish that on a base when you get this thing going. Seeing that England is so modest in its desire for a military establishment, I think you might get it up to date quicker. It is no good arguing over an expansion or demanding an expansion of the policy till you have accomplished a policy you have already laid down. That seems to me to be the essential thing; get that expeditionary air force on its establishment, bring it up to it, and then talk about further expansion. As far as the military side is concerned, as I said, one is apt to be kicking the wrong football. Then if we turn to the naval side there remains the fact that it has not been specified what they are going to do, and I doubt whether it ever will be specified what they are going to do. We have heard it said that they state how many warships are going to be built, but that is rather for the reason that one can see those warships and count them more or less conveniently, but we do not know in this country how many submarines we have, and I doubt if anybody (outside Germany) does. We need more aeroplanes in the Navy and more dirigibles, dirigibles particularly; there is no doubt that these have to be constructed in this country—they are very expensive—and also sheds. I suppose that you cannot put up a dirigible shed under nine months, and the shed will cost £50,000, and the dirigible that or more. At any rate, they have to be allowed for, and it is important that we should construct them in this country. I wish I had heard to-night what is going to be said about this side of the business. I think the nation will be disappointed, and that people ought to express their disappointment if Mr. Winston Churchill in Parliament does not provide that other half a million. I do not say that Colonel Seely has provided it in anything but the figures put down on paper, because in that half million most of it is money that has got to be spent every year for paying the men. I do not know what you call it; it is down as establishment, and it represents salaries and the like, £120,000. That sum represents five times the sum allocated in last year's estimates. It does not seem to have been commonly pointed out that £28,000 was put down in last year's estimate for establishment. How the establishment was paid on £28,000 I do not know, but the fact remains that it is now multiplied by five, and we are faced with the situation of having the establishment multiplied by five, and nothing else multiplied by even one as far as I can see. That is a small item

that wants attention. We are purposing to pay salaries at the rate of 5 to 1, and there is no proposed capital expenditure to accompany it. What we are paying salaries for is not clear. It seems to me that a lot of that quarter of a million is not likely to go to experimental work. With much that has been said to constructors and their hardships, I am in thorough accord, because Colonel Seely in that Memorandum said, amongst other things, it was essential or desirable—I forget which word he used, it ought to have been essential—that the conditions of this country, or rather the Government requirements, should absorb the energies of the industry. I forget the exact words, but the sense of it is what I say. In that Memorandum it says it is desirable that the Government programme should absorb the resources of the country, meaning, I imagine—I took it in no other way then, and I take it in no other way now—the industrial resources of the country. I think an insufficient attempt has been made on the part of the Government as a Government; I am not speaking of military or naval requirements, but the Government and its interest in keeping alive that industry. Some action should be taken. I am not in favour of the scheme of making the air service an independent service, I do not know anything about the military or naval business, but it seems to me that it would be impossible to work it and to administer it as a separate service. It might be of considerable utility if there were some independent organisation, not necessarily official, that made it its business to see that the air service was adequate.

Then there is another point, and that is this: we look at France and Germany, and naturally see the air forces associated with military work. The safety of a country depends fundamentally on guarding its approaches. The approaches in France and Germany differ from ours; theirs are over land whereas ours are over sea. It is natural that all warlike development should be associated abroad with the Army. If it was a question of cavalry or artillery we should feel less concerned with developments. I want to draw your attention to one fact, namely, that an air service can be transferred, or in principle at any rate transferred, from military work to naval work. Therefore it behoves this country not to associate air development abroad with military work, but to regard it as an independent thing. By that I do not mean it is something that should be independently organised, but something which can at any moment be transferred for oversea work. I think that is important, because if France and Germany were developing their air forces ostentatiously in connection with their navies, we should be excited about it at once, and I think people are all apt to feel—I am now speaking of the utterances of Ministers—because things are going on abroad in connection with armies that therefore they are not affecting us as much as I personally think they should be regarded as so doing. To that extent we should develop on a larger scale in England, and particularly should it be done in connection with the Navy with hydroplanes and dirigibles and the like. It might well be, especially in Parliament, that some special interest can be aroused in respect of forcing on the attention of Ministers the need for absorbing the resources of the country and keeping the scale in size of aeronautical work in this country up to the level of work abroad irrespective of the Army or the Navy.

Mr. Mort: I am afraid that my interest is only academic, because I am an engine builder. I think it is more pleasure to listen than to speak, but the thing that interests me is the forthcoming engine test. I hope that sufficient time will be given between the announcement of the test and its nature, and the time it takes place so that there may be time to produce an engine that has a chance of success. In the War Office trials last year there were many engines that were pushed out of the competition simply because there was not enough time for them to be completed, and it does not seem to me that that is the aim and object of the competition. I hope that some effort will be made to get the fair time which we shall require.

Dr. Mullineux Walmsley: I am not a manufacturer of aeroplanes or directly connected with the manufacturing industry, but I have been invited here to-night by your courtesy to take part in a discussion upon the effect of the Government aeronautical programme upon the technical industry. It seems to me in that title there lies a very serious question, and in fact the whole question here. We are in this position; we have not got the Government proposals before us; we have only a part of those proposals, that being the part connected with the Army. As a scientific man and having some

knowledge of aeronautics, and also of public affairs, it seems to me that the present position is an extremely critical one indeed. That a Cabinet Minister should get up in Parliament and speak complacently of having 100 aeroplanes, some of which we have heard to-night are certainly not suitable for use, is very lamentable when you consider the figures that we get from other countries. Those figures also, we have every reason to suppose, do not tell us the whole truth, and even amongst the present aeroplanes I should like to ask how many should be scrapped as being obsolescent. That word is known to engineers very much indeed, as obsolescence in the aeroplane industry is at the present time much more rapid in its effects than it is in any other part of engineering industry. Not being a manufacturer, if you will not rule me out of order I would like to say a word or two about what has been referred to by the Minister, namely, the importance of the personnel in the development, and especially as Colonel Seely pointed out, that it is important we should have highly trained mechanics. But what is there in the whole of his speech to show that he is doing more than lip service to the declaration, because there is no suggestion from the beginning to the end of his speech as to how the mechanics are going to be trained, and the position is a very serious one. It makes one doubt indeed whether the Ministers are really in earnest. The responsible Minister admits he wants highly trained mechanics, and he issues a circular, which I hold in my hand, offering 4s. a day. That I think any manufacturer present will tell him will not procure a mechanic such as he mentioned in his speech, for he has to compete in this respect not simply with the ranks of the Army, but the open market in the competition for these mechanics, and at the present time it is common knowledge that there is a boom in the engineering trades and a small number of unemployed skilled mechanics. You are not going to attract them by wages of that kind. But there is in my opinion a point in connection with this business which seriously affects the question of getting a supply of trained mechanics, and that is, that at the present moment the rank and file of the engineering industry do not regard it as a paying proposition, and you cannot get men who will consent to be trained. I speak, as you probably know, with some knowledge of this subject. I happen to be the principal of one of the largest engineering polytechnics in London, and we have had for some years classes in aeronautics, but the rank and file do not believe in those classes and will not come into them. We can fill our classes for automobile engineers, we can fill our classes for electrical engineers, and for other things, but the aeronautical classes do not fill. It seems to me that one of the reasons is, as has been stated to-night, that the market is limited and the Government is the only one that buys at the present time. That is fully recognised, but when we hear a manufacturer get up in this room and tell this audience that he has had to dismiss those mechanics he has trained, can we wonder that the young engineer does not come to our classes to be technically trained? It will be admitted and is clear that this new industry does call for a highly technical and special training on the part of mechanics as well as on the part of designers. Mechanics have in this industry, as it were, to get out of themselves. They have been trained in engineering shops, but they have to get another kind of knowledge, a very special knowledge, in fact often knowledge which does not yet exist, at least what has not yet been published, and their engineering imagination has to be trained as well as their hands and brains. The institution I have the honour to preside over is placed in London within a mile of St. Paul's, and this is my experience, that the Government has got to encourage the rank and file as well as the manufacturers if they are going to make a success of this business. Talking of giving a manufacturer an order for fifty as the result of the competition—and he may not get the order—is to my mind not a reasonable proposition at all. Some of the manufacturers in this room may know, if they have to find plant for turning out a machine in bulk they have to spend a considerable sum on plant, and unless they can show a continued quantity of orders it is not worth their while to plank down the capital. That is the manufacturers' side of the question which has been expressed to-night. It may be said that all this is in the interest of economy. I would point out one or two things in that direction. It is quite true that you may squander money by rashly giving orders, but there are two things you can squander: you can squander money and you may get it back again, but there is the other thing which if you do squander you will never get back at all, and that is time, and it seems to me that time is the problem at the present moment. We are at the mercy

of the aeroplane forces of the Continent. I speak in the presence of experts, military and naval perhaps, and I speak as an outsider, but I take it that that proposition will not be contradicted that at the present moment we are at the mercy of the aerial fleets of the Continent. I am speaking here with no axe to grind because I am not a manufacturer ; I do not want orders, but I am speaking as an Englishman, and I think that one of the richest services that this particular Society could do to the industry and to the country at the present time, is to urge upon the Government the time is flying, water has run under the bridges, and unless something is done soon and done upon a scale compared to which the proposals of the War Minister are insignificant, we *may* meet with a very grave disaster. I am not an alarmist, but that is the result of my investigation.

General HENDERSON : Some remarks have been made to-night which I might touch up a little. Mr. Berriman confesses himself rather mixed about the personnel, why it had been multiplied and other things had not, but the cost of the personnel is multiplied, because the personnel is multiplied. We have now more than five times as many officers and men as we had at the beginning of the year, and it is a simple calculation when they cost very much about the same in the different ranks. Of course, if you have more of them, they cost more. I think everything else in the Estimates, as far as I remember, has also been increased, with the exception which I know you will welcome very gladly, that so much will not be spent upon land. I think he understood that the other things had not increased, but I think they have, everything except land. Then Mr. Joynson-Hicks drew attention to the advantage of constructors getting steady orders instead of spasmodic orders. Last year, as everybody knows, there was an insufficient provision made at the beginning of the year for the needs of the Flying Corps and if you only get spasmodic money, you must give spasmodic orders. If you have not steady money you cannot give steady orders. The money we have been able to get has been largely borrowed and saved from other things, and certainly for a considerable interval we were unable to give orders because we had not got any money. Then Captain Wood mentioned the allotment of the Factory. The allotment of the Factory includes nearly everything we buy, including aeroplanes, all the stores for the Royal Flying Corps. It is not published what the allotment is inside that, but it is clearly laid down, and there is no chance of anybody stealing anybody else's money. He made a suggestion also, which seemed to be backed up, that if we could arrange certain tests and announce that we would buy machines that met those tests, that would be a satisfactory way of making a purchase. I quite agree theoretically that it would, but I do not know how to draw up a test that is going to be a satisfactory one, because an aeroplane is not like anything else. After you have bought it, a man has to fly it, and you may make as stringent a test as you like, and a constructor will make a machine to pass those tests, but he may not make a machine that a man wants to fly. I do not know how you are going to get over that. We do not want to try and force on officers flying machines they do not like, and I do not think that any large purchase could properly be made of aeroplanes if the officers were disinclined to fly. We may have difficulties enough in the future, when we get designs, which I hope we shall, of aeroplanes that are fitted for warfare ; it will probably be difficult to fly some of them, but while we are working with purely flying machines I think it would be most disadvantageous to the service generally if we did not consider the wishes of the officers who have to fly these machines, and how to get over that test, I do not know.

We have tried to distribute orders, in the first place in order that we may be thoroughly acquainted with the machines we might buy, and until we were acquainted with them and the officers who had flown them, we were not inclined to purchase more until we had got their opinion. At present I think we shall stick to that. A test and a promise of purchase seems to me the proper way of buying engines, which are on a different footing in that respect. I should think that the competition which has been announced will be on those lines.

The policy of the Government, as far as the Army is concerned, has been declared public ; a White Paper was published which set it forth, and as far as I know it has not been departed from in any material detail. Certainly no mention was made of any time

limit in which this programme was to be carried out, and that seems a natural point for criticism, although I think it is more suited to political discussion than to expert aeronautical discussion.

The CHAIRMAN: As nobody else is going to speak, I will offer a few remarks, and they will be very few. In the first place, I think we have had an interesting and, I believe, a useful discussion to-night, but it has been very difficult to have a proper discussion with inadequate knowledge of the Government programme. That, I think, we have all felt.

I am sure that we are all very glad that General Henderson came here to-night. I have had a very long experience with the War Office, and I should like to take this opportunity in public of saying how much I admired the way in which the War Office have sprung up during the last year; it is very remarkable. It is a very difficult place to get anything done, because of the interweaving of the different departments and the time it takes. I think the organisation of this Flying Corps, and another associated with the Navy, all done within a year, concluding with a remarkably efficient corps, is a very remarkable feat, and it is to the greatest possible credit of the old War Office. I am sure that the Society is very much obliged to these gentlemen who have come here who are not members: to Mr. Joynson-Hicks, for coming here, because we all know he takes a great interest in the subject, and we expect him to help us on every possible occasion: to Dr. Walmsley too, I am sure we are very much indebted to him.

Mr. JOYNSON-HICKS: I am sure that we should not part without thanking our Chairman, and as one of the guests to-night, I have much pleasure in proposing a very hearty vote of thanks to him for presiding.

The motion was carried unanimously and the meeting terminated.

THE THIRTEENTH Meeting of the Forty-eighth Session of the Aeronautical Society of Great Britain was held on Wednesday, 21st May, 1913, at the Royal United Service Institution, Whitehall, S.W., at 8.30 p.m. Major-General R. M. Ruck, C.B., Chairman of Council, presided.

The CHAIRMAN: My Lords, Ladies and Gentlemen: We have met here this evening to honour the memory and work of that great American aeronaut, Wilbur Wright, who with his brother Orville first made the conquest of the air a possibility and not a mere dream. I would suggest that the large attendance at this Meeting indicates the degree of respect and admiration in which that memory is held. We recollect with pride that Wilbur Wright was a Member of this Society. There are a few present here this evening to whom he was an intimate friend; all the greater is their loss, for in him was a charming companion unknown to the outer world.

It was a bright inspiration of one of our Members of Council (Mr. Clarke) to suggest this annual lecture, identifying as it does the keynote of Wilbur Wright's work, namely, his most painstaking scientific investigation and research, with the chief objects for which this Society exists.

The success of the Memorial Fund is largely due to the personal efforts of Mr. Griffith Brewer, and although organised by and held under the ægis of this Society,* it is only proper that I should mention here that this Memorial Lecture has received the generous support of the Royal Aero Club and of many public men and others outside the Society: it thus may be said to be a representative British tribute in Wilbur Wright's honour. In this spirit we heartily welcome the presence, amongst our guests, of several representatives of America. They are of our own kith and kin, and I am sure that we rejoice equally with them when one of our common stock has distinguished himself so pre-eminently as Wilbur Wright.

I have emphasised science as being the keynote of the work done by Wilbur Wright and his brother Orville: our main object, therefore, has been to render the present occasion worthy in the scientific sense, and I think you will agree with me that we are fortunate indeed that Mr. Horace Darwin should have consented to deliver our first Memorial Lecture. Of all British names, none could better typify the original thought and research that it is our purpose to commemorate, than that of Darwin, and it was a lucky day for scientific aviation when one of the family turned his attention in this direction. Mr. Horace Darwin is a Member of the Government Advisory Committee for Aeronautics, his own special study being the design of scientific instruments, and it is upon this subject more particularly that he has elected to lecture. This is in accordance with the general wish of the Society that the lectures should be a modern contribution to knowledge of the kind that Wilbur Wright himself would most have appreciated.

At the present time I suppose that there is no subject of greater concern to those engaged in practical aeronautics than in the evolution of scientific instruments that shall tend to reduce the element of personal frailty. With this introduction I will now call upon Mr. Horace Darwin to commence his lecture.

# SCIENTIFIC INSTRUMENTS, THEIR DESIGN AND USE IN AERONAUTICS

## BEING THE FIRST WILBUR WRIGHT MEMORIAL LECTURE

By HORACE DARWIN, M.A., F.R.S.

"The chief cause of failure in operation is the ill determination and measurement "of the forces and actions of bodies."—*Novum Organum, Francis Bacon.*

To be asked to give the first Wilbur Wright Memorial Lecture is an honour and a responsibility, and I hope that what I shall say may be of some interest to the many able men now working at the science of aeronautics.

* (See AËRONAUTICAL JOURNAL, No. 63, pp. 146-7.—ED.)

No Memorial Lecture is required to make us all admire the character of the man, his brilliant engineering work, the scientific method by which he obtained his results. Each step forward was secured by careful reasoning based on former trials ; each step was tested separately ; all available data were used. An account of the method and results of his original experiments has not yet been published, and would be of extreme interest. May I express a hope, which I know you will share, that when the appropriate time comes it may be published ?

All Wilbur Wright's work was done in the closest co-operation with his brother Orville. We do not know how much each did, and we do not want to know ; Orville probably says that Wilbur did most, and it is equally probable that Wilbur would have said that Orville did most. At any rate we know that together they did a very great piece of work.

**Natural and Artificial Flight and Locomotion**

The Wright brothers made careful observation of the flight of birds, and found their observations valuable. It is interesting to consider the resemblance and differences of the manufactured aeroplane and the living bird. The resemblance may be simply the result of copying the bird, or it may be that similar designs have been arrived at independently by birds and men. The wings of both are roughly the same shape : of wide span, and narrow in the direction in which the bird flies ; both have concave wings with thick leading edges. In many aeroplanes hollow spars are used like bones and like the quills of the feathers of birds. We copy plants also in this respect, for they too have learnt the economy of material in the use of hollow spars. The bodies of airships are similar in shape to the bodies of swimming animals, with the greatest width towards the head. The bodies of birds are of similar form.

These resemblances are remarkable, but there are great differences. The Wright brothers found no biplane bird to copy and did not flap their wings. No flying animal uses a continuously rotating propeller to drive him forward on soaring wings, and it is perhaps hardly too much to say that if birds only knew how, they would now copy the Wright brothers. Muscular action and the circulation of the blood however, put supreme difficulties in the way of the development of the continuous rotation of a part of an animal. Cranks and connecting rods, as well as rotating valves to allow the circulation of the blood, would be required. No animal has succeeded in developing wheels instead of legs, although their development might have enabled him to run about with less consumption of fuel, anyhow in a country with good macadam roads. There is a beetle who has made use of something in the nature of a wheel. He collects his food (manure) and carefully fashions it into a ball larger than himself, and then rolls it along to a convenient place, buries it, and lives on it at his leisure. This is not a wheel, but it has many of the advantages of a wheel, and we may consider this beetle as the pioneer transport engineer.

The development of the power of flight in birds has been so slow that we cannot realise the time taken, or form the roughest estimate in years ; but the perfection of these adaptations and the beauty of their skill, strength and movement must strike anyone who has ever watched their flight. Some less advanced animals have only learnt to glide, and are now in the same stage of development as the Wrights were a few years ago. Perhaps these gliders developed more slowly or perhaps only began to learn the art many ages after birds had learnt to fly. A few plants also have developed wings to their seeds so that they can glide away to more suitable places for germination and growth.

The evolution of those remarkable flying animals, the Wright brothers, has been enormously more rapid because it has not depended on the method of trial and error, and because each trial does not correspond with the lifetime of an individual. But the difference is even more far-reaching, since the material on which they worked was knowledge, or, in other words, the experience of mankind handed down from one generation to the next. And more important still, their mental powers enabled them to test the accuracy of this knowledge and to increase its amount.

It is interesting to note that it is the opinion of biologists, that birds, bats, fishes and insects all learnt to fly independently ; inheritance from a common stock played no part in their development. There are no large flying animals now and the great majority in existence are extremely small. These facts would lead us to expect difficulty in making a very large aeroplane, and theoretical considerations confirm this view.

The Ostrich, the largest existing bird, standing 8 ft. high, has no power of flight; nor had the Moa of New Zealand (now extinct) standing 12 ft. high. It is probable, however, that both had flying ancestors. The only vertebrate animals which developed flight during fossil times were Pterodactyls. The largest of these had a body a little larger than a swan and a span of wings of over 22 ft. This is less than the smallest aeroplane, but large compared to an Albatross, the span of whose wings is over 11 ft.—probably exceeding that of any other bird. The whole family of Pterodactyls has long ago become extinct.

On the other hand there is a very small spider who makes a kind of flying machine out of the simplest materials. In the autumn he desires to emigrate, and as he is very small a land journey would be slow and difficult. He selects a calm and sunny day, on which we should expect to find local upwards currents of air; he climbs to the tip of a blade of grass and spins a thread which is blown out by the wind, and at the right time he lets go and is carried away by the wind, he knows not where. This clearly could not be done by a large animal. But we had better not attempt to copy flying animals too closely. We shall learn nothing from the spider's success in aeronautical engineering. Would it not also be just as great a mistake to try and fly with flapping wings as it would be to propel a ship by a flapping tail like a fish, or to make a motor-bus trot about the streets on four legs? I think it would, but my ancestor Erasmus Darwin thought differently. In "The Botanic Garden" he wrote:—

> Soon shall thy arm, UNCONQUER'D STEAM! afar
> Drag the slow barge, or drive the rapid car;
> Or, on wide-waving wings expanded bear
> The flying-chariot through the fields of air.*

A few years ago I did not believe that we should see the essential part of his forecast about flying fulfilled, and I may be just as wrong about the waving wings.

## The Design of Scientific Instruments

The subject of my lecture to-night is Scientific Instruments—their use in connection with flying—and some general considerations with regard to their design. The Wilbur Wright Memorial Lecture will be given annually, and I believe the most useful results will be obtained if the lecturer is allowed considerable latitude in the choice of a subject. All I ask is to be allowed to speak on a subject at which I have worked for many years.

## Instruments used in Aeroplanes

It is important to realise beforehand the difficulties of using instruments on aeroplanes during flight and the errors that may be introduced in the readings. The aeroplane shakes, it does not remain level, and is subject to acceleration in all directions. The instrument should be so designed as not to be affected by any of these disturbances. A vertical accleration has the same effect as a change in the amount of the downward pull due to gravity, the tilting of the aeroplane changes the direction of the downward pull, with regard to the instrument. A lateral or longitudinal acceleration has the effect of altering both the direction and the amount of gravity. But vibration is a greater difficulty still. The hand of an instrument may move so much and so rapidly that it is difficult to estimate the mean reading on the scale, and sometimes it is quite impossible to do so. And this may happen when the quantity which is indicated by the position of the hand only varies slowly and by small amounts.

Consider a part of an instrument that can rotate about a vertical axis, and suppose that its centre of gravity is not on the axis, then a sudden lateral movement of the whole instrument will tend to rotate the part relatively to the instrument, the side-ways force will act at the axis, and the resistance to this force due to inertia will act at the centre of gravity of the part. The farther the centre of gravity is from the axis, the greater this tendency to rotate; the tendency will also be greater the greater the mass of the moving

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* Erasmus Darwin was born 1731 and died 1802

part. To make this tendency small, the centre of gravity should be as nearly as possible in the axis, and the weight should be small. If, however, the part is made very light it may bend and vibrate in itself from the forces acting on it from its inertia. In fact the part should be balanced, light, and stiff.

Good balancing and lightness also reduce the tendency to movement due to tilting or want of level in the instrument; if the axis is no longer vertical the centre of gravity will try to get to the lowest point, and the piece will tend to turn.

In a compass as ordinarily made, the condition of balance cannot be fulfilled. The magnet rests on a steel point and is horizontal, and its centre of gravity is below the steel point. The force on the North Pole acts in a downward direction towards the North, and the force on the South Pole in an upward direction towards the South, and the magnet is made to rest in a horizontal position by arranging that the centre of gravity of the magnet is between its South end and its centre. It is below and to one side of the point about which rotation takes place. Hence a side-ways movement must start it swinging. In Fig. 1 the distance of W from the steel point is greatly exaggerated. The magnet and card in aeroplane and ship compasses are usually surrounded by a liquid, so that any vibration which may be caused by its want of balance is rapidly reduced.

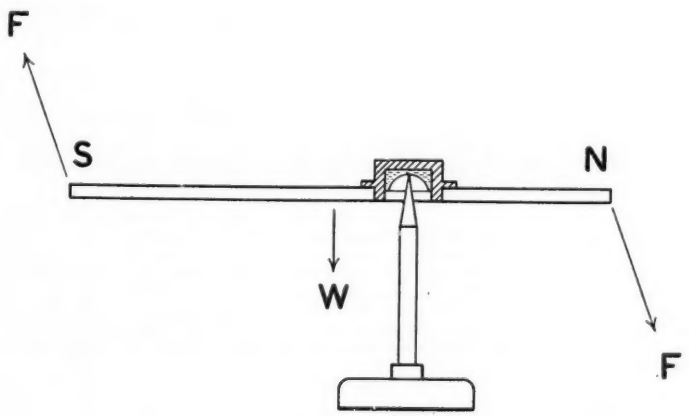


FIG. 1

Instruments on aeroplanes should be damped, using the word to damp in the sense of "to dull" or "to abate the motion of." This damping is specially important if it should happen that the rate of vibration of the whole instrument should agree with the natural rate of vibration of the moving part. When this happens with an undamped instrument, the vibration is excessive. Damping is also required in cases where the fluctuations in the quantity to be measured are rapid; it may then be difficult to read the instrument, and the excursions of the hand may indicate a much greater amount of variation of the quantity than really takes place. If the mean reading is required the instrument must be damped and the damping should be of a particular kind, which we will now consider.

The essential features of satisfactory damping are that no force should be applied to the moving part whilst it is at rest, but that as soon as it moves a force should act opposing the movement. Friction at the joints damps the instrument, but it does not fulfil the above conditions, and is bad. The force should be small when the movement is slow, and it should increase when the movement becomes more rapid. The most usual method is to immerse the moving part, or a paddle fixed to it, in a liquid more or less viscous, or the paddle can be replaced by a fan in the air. Another method is to damp by the movement of a copper plate between the poles of a magnet. If a Pitot tube is used, the flow of air through the connecting tubes damps the instrument.

Mr. A. Mallock has pointed out that in order to obtain a true mean reading with an instrument the damping force should be proportional to the velocity of movement of its index. When the damping force varies as the square of the velocity there may be no error or there may be a considerable error. We will take a particular case. Suppose that the quantity to be measured remains at 80 for 2/10 second, and then suddenly increases to 140 and remains at that amount for 1/10 second, and then it goes back to 80 and remains at that amount for 2/10 second, and that this rapid oscillation goes on indefinitely. Suppose also that the instrument is damped by a force which varies as the square of the velocity of the index, and that it is so much damped that the hand appears to remain at rest. The reading of the instrument will be 92 and the true mean in reality is 100, so that we have an error amounting to 8 per cent., by no means a small error. The diagram Fig. 2 gives the supposed variations of the quantity as it would be recorded on a moving sheet of paper, and gives the true mean and the instrument reading.*

In the magnetic method of damping, the force varies as the velocity and the true mean is obtained. With liquid and air damping, the force varies as the square of the velocity, unless the movement is extremely slow, when it varies nearly as the velocity.

## Speed of Aeroplanes

The speed of the aeroplane through the air is usually given by reading the position of some form of index on a scale graduated in miles per hour. A Pitot tube and a manometer are often used.

The principle of the Pitot tube is very simple (Fig. 3). If the open end of a tube faces the wind, the air wants to pass down the tube, and if the tube is closed at the other end the air pressure is increased in the tube, and this increase of pressure is a remarkably accurate means of measuring the velocity of the wind. This method is used in Dines' Anemometer, and for measuring the velocity of the air in the wind channel at the National

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* The general proposition is as follows :—We will suppose that the divisions on the scale are equally spaced and that each space corresponds to an equal increment in the quantity to be measured; also that the quantity to be measured is given by a reading $a$ on the scale and that it suddenly changes to $b$. The hand will begin to move from reading $a$ to reading $b$ with a velocity $v$, and we will assume that the damping force varies as $v^2$. This damping force must be equal to the force acting on the moving parts of the instrument which tend to move the hand from scale reading $a$ to $b$. This force in an instrument with a uniform scale will generally be proportional to the difference of $a$ and $b$, and we will assume that this is the case. Hence the force on the hand varies as $b - a$, and this is balanced by the damping force. That is

$$b - a = kv^2 \quad (1)$$

where k is a constant.

Now consider what the amount of movement will be for a time $t$ which is so short that $v$ may be supposed to remain constant. Let $s$ be the space moved through by the hand. Then—

$$s = vt = \frac{\sqrt{b - at}}{\sqrt{k}} \quad (2).$$

Now take a case similar to that indicated in Fig. 2 so that the quantity to be measured remains at $a$ for the short time $t$ and at $b$ for the short time $t'$. The true mean

$$M = \frac{ta + t'b}{t + t'} \quad (3).$$

Let $m$ be the mean reading with the square law damping. Then in order that the hand should appear to remain at rest at this point or rather that it should have a small and equal oscillation on both sides of it, the amount it moves up during $t$ must equal the amount it moves down during $t'$. From equation (2) we get :—

$$\sqrt{m - a}\ .\ t = \sqrt{b - m}\ \ t' \quad (4).$$

so that

$$m = \frac{t'^2 b + t^2 a}{t^2 + t'^2}.$$

It follows that $m$ can only equal $M$ when $t$ and $t'$ are equal.

If the damping force varies as v and not as $v^2$ equation (2) becomes $s = \frac{b - at}{k}$ and equation (4) becomes $\sqrt{m - a}\ t = \sqrt{b - m}\ t'$ $m = \frac{ta + t'b}{t + t'} = M$ for all values of $t$, $t'$, $a$, and $b$.

Physical Laboratory. In 1903 Dr. Stanton read a paper before the Institution of Civil Engineers (*Proc. Inst. C. E.*, vol. clvi. p. 78) proving the accuracy of this method of measuring air velocity. Dr. Glazebrook tells me that improvements have recently been made which give even more satisfactory results. The delicate measurement of the air pressure necessary for the most refined work is made by the Tilting Water Gauge designed by Professor A. P. Chattock and Mr. J. D. Fry. This is a laboratory instrument of the highest order of precision, and is far too delicate and accurate to be used on a flying machine. It is a difference of pressure that has to be measured—the increase of pressure in the tube, above the air pressure outside—and a second tube transmits this pressure (the static pressure) to the manometer. It is found by experiment that changes in the size of the opening of the Pitot tube, or the thickness of the tube, or the bevelling of its edge, make little or no difference in the pressure. With the opening of the static tube it is different, and its design is important. In the design now adopted at the National Physical Laboratory the pressure obtained is almost exactly what we should expect from

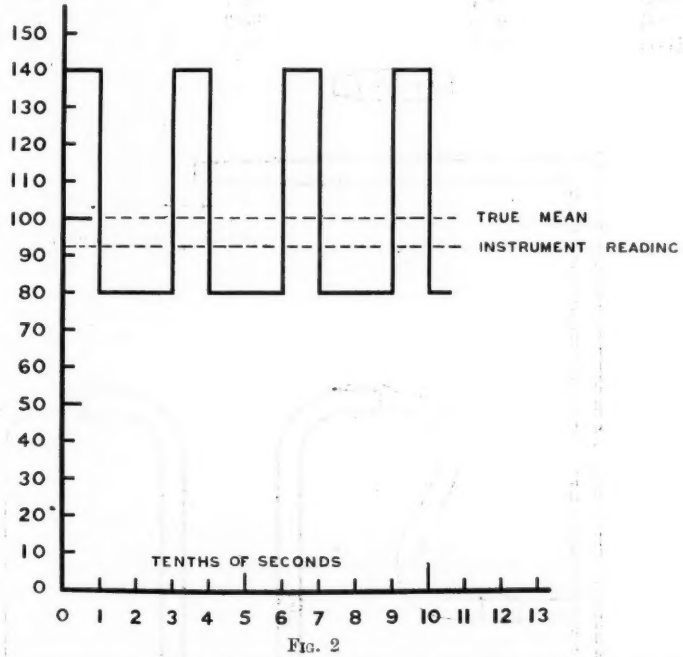



FIG. 2

theoretical considerations. This is an advantageous simplification and this form of Pitot tube should be used for all the most refined measurements. But the static tube can be so made that it will give a pressure below the true static pressure and the Royal Aircraft Factory have made use of this and have increased the manometer readings by 20 per cent. in order to give a more open scale.

The tubes transmitting the pressure can be carried a considerable distance to allow the manometer to be placed in a convenient position for reading; this is often of great importance. If it is found advisable to have a large amount of damping in the manometer it is best to have long tubes of large diameter. This gives the correct form of damping. Short tubes of small diameter will also give a large amount of damping, but in this case the damping force will vary as the square of the velocity of the air in the tube, and the reading will not necessarily be the true mean. For the same reason it is inadvisable to cause damping by throttling the passage of the air by closing a valve, or by means of letting it pass through a small hole in a plate.

If a Pitot tube speed-meter gives the correct speed when flying near the ground level, it will not be correct when flying at a great altitude. The error is caused by the change in the density of the air. As you mount the air becomes less dense because the atmospheric pressure is reduced, and more dense because the temperature falls.

In the following table the actual speed is assumed in all cases to be 100 miles per hour. The temperatures given in the last column are taken on the usual assumption that there is a fall of 1° F. for every 300 ft. rise above the earth's surface.

| Height in ft. | Speed readings temperature constant. | Speed readings temperature falling with height. | Assumed temperature. |
|---|---|---|---|
| 0 | 100 | 100 | 50° F. |
| 1000 | 98·3 | 98·6 | 47° F. |
| 2000 | 96·5 | 97·1 | 43° F. |
| 3000 | 94·7 | 95·7 | 40° F. |
| 4000 | 93·0 | 94·3 | 37° F. |
| 5000 | 91·3 | 92·9 | 33° F. |

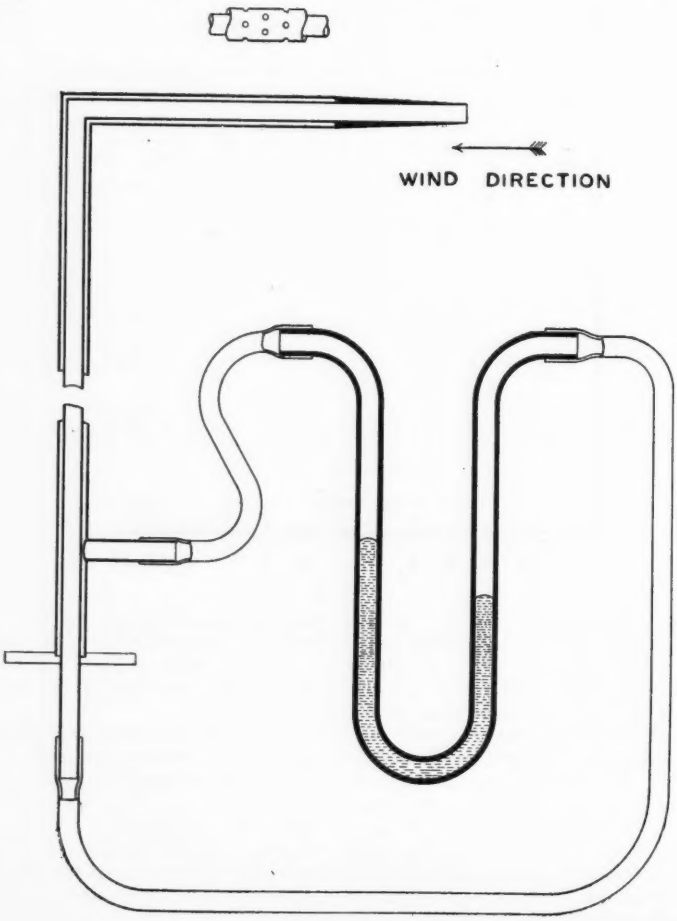



FIG. 3

These corrections were made in the air speed measurements at the Military Aeroplane trials in 1912 when obtaining the gliding angles of the various competing aeroplanes.*

The simplest form of manometer is a U-tube containing a liquid (see Fig. 3). The difference of the level of the liquid is then a measure of the difference of the air pressure in the two tubes. For use on an aeroplane this has two drawbacks: the scale is not open enough to read the speed easily and accurately and tilting of the aeroplane causes an error. Mr. Short of the Royal Aircraft Factory has designed a manometer which overcomes both these objections. It is in effect a U-tube manometer; he uses two liquids of different densities which do not mix, and thus obtains a more open scale (Fig. 4). One tube is placed inside the other, and this overcomes the chief error due to the tilting of the aeroplane, leaving only a small secondary error of no importance. If the manometer is placed 10° out of the vertical, this secondary error will make the reading be 101·4 miles per hour instead of 100. Another form of manometer designed by Mr. Short, using one liquid, and still eliminating the chief error due to the tilting of the aeroplane, is also shown in Fig. 4.

If the aeroplane has an upward or downward acceleration or is changing its direction there is an error.

An aeroplane flying at 100 miles per hour in a circle of 1400 feet radius will make one complete turn in one minute, and the banking of 26° will prevent side-slip. In this case the manometer connected to the Pitot tube will read 94·5 miles per hour instead of 100, the correct speed.†

If the vertical acceleration is one-tenth gravity or 3·2 ft. per second, the manometer will read 5 per cent. too low or too high according as the acceleration is downwards or upwards. These errors due to vertical acceleration or flying in a circle are not large and they will be the same in any manometer in which the air pressure is balanced by the attraction of gravity on a liquid or a weight. If a spring is used these errors may be eliminated.

If a Pitot tube is fixed to the tips of the wings of an aeroplane and it is flying in a circle, the speed of the outer wing tip is greater than the speed of the inner wing tip. If these Pitot tubes are joined together by a tube there will be a greater pressure at one end of the tube than at the other, and at first sight we should expect that there would be a flow of air through the tube from the outer to the inner wing tip. But this is not the case, because the aeroplane is moving in a circle and there will be centrifugal force acting on the air in the tube. This will tend to make it flow outwards and will exactly balance the tendency of the air to flow inwards due to the excess pressure in the Pitot tube on the outer wing tip, and there will be no flow through the tube. If there is a side-slip this statement is only approximately true, but if there is sufficient banking to prevent side-slip it is true. If both a Pitot and static tube are fixed at the tip of one wing and are connected to a manometer at the centre, its reading will give the velocity of the wing tip. The centrifugal force in this case will act equally on the air in both tubes, and as the manometer measures their difference of the pressures, the centrifugal force will produce no effect on the reading. When flying in a circle the velocity of this wing tip is not the velocity of the centre of the machine. This difference is usually not large, but if it is thought advisable that the manometer should give the velocity at the centre, two Pitot tubes can be used, one at each wing tip, and both be connected to the manometer, the static tubes also both being

* Mr. Harris Booth has pointed out to me that, as is well known, the lifting power of the wings becomes less with increase in the altitude for the same reason as the pressure on the Pitot tube is reduced. It follows from this that when an aeroplane flies in a horizontal direction and with the same angle of attack, the manometer reading will be the same at any altitude although the speed will differ. In those cases where a speed meter is used chiefly to give warning when the speed is lower than that required to support the aeroplane the error in the actual speed reading must be considered as an advantage since it is an automatic correction for the change of density of the air. But when distance covered is being considered the error remains.

If the engine is stopped when flying in a horizontal direction the machine must be made to dive; this change of level will cause a small error as explained later.

† When flying in a circle the wings have to support a greater force than that due to the weight of the machine, the force being the resultant of the weight and centrifugal force. Mr. Harris Booth has pointed out that the controlling force due to gravity which acts on the liquid is also increased in the same ratio. It follows that what is an error in the speed reading again becomes an automatic correction when the sustaining power of the wings is considered. If the air pressure is measured by a spring and not by gravity this automatic correction does not take place.

connected. The manometer will then give the mean of the speeds of the wing tips, that is the velocity at the centre. This is done for accurate speed measurements at the Royal Aircraft Factory.

**Yaw-meter**

The word Yaw is here used in the sense of " to bend or deviate from a straight course." But the name does not accurately describe what the instrument measures. If the air is at rest it measures the angle that the direction of movement of the aeroplane makes with its keel, or how nearly it is moving in the direction of its length. Side-slip is thus

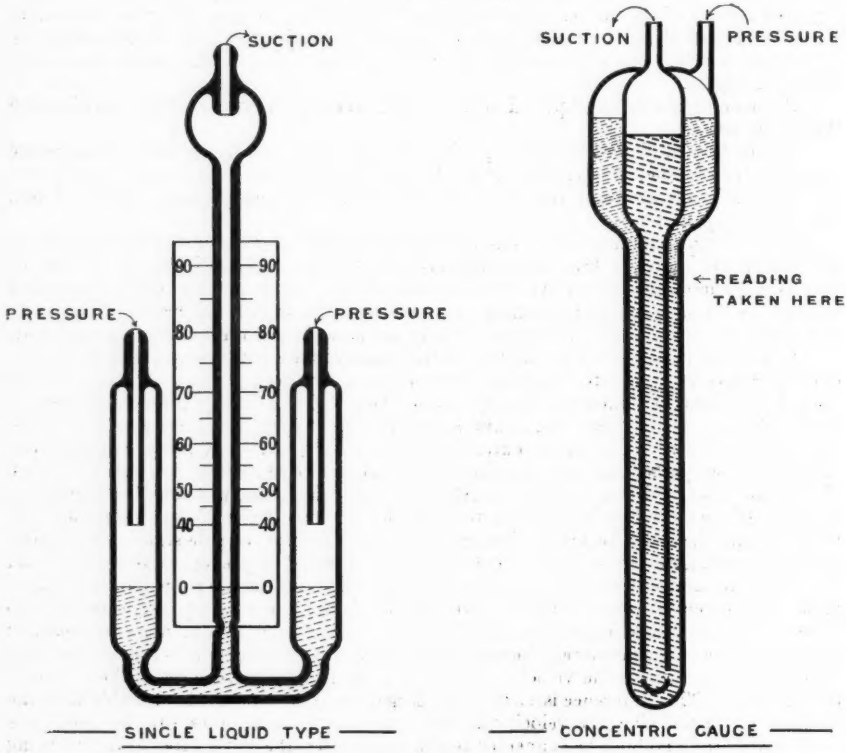



FIG. 4

measured by it. If we consider the aeroplane at rest and the air blowing against it, it measures how nearly the direction of the wind is head on. If an eddy in the moving air meets the aeroplane, the direction of the wind will change and this will be indicated. A wind-vane carried by an airship or aeroplane would also show how nearly the movement was head on in the same way as the Yaw-meter. But the wind-vane would be difficult to read when placed in a position free from eddies in the air caused by the air-craft itself. With the Yaw-meter the dial and hand can be placed in a convenient place for observation.

Two Pitot tubes are made like the letter Y (see Fig. 5) with the openings at the tops of the two arms. If the wind blows symmetrically to the two tubes the pressure will be

equal in both. But if the direction of the wind changes it will meet the opening at the end of one tube more nearly in the direction in which the tube is pointing, and the pressure will be increased. The opposite will take place in the other Pitot tube and the pressure in it will be diminished.

The pressure from these two Pitot tubes is taken by two pipes to the indicating apparatus which can be at any convenient distance away. Each tube is connected to a circular box the top of which is an air-tight flexible diaphragm which can move outwards. A rod is connected to each diaphragm, and these rods are pushed outwards by the air pressure.

The hand indicating the angle of Yaw, that is the angle at which the air meets the Y Pitot tube, is pivoted about the point O, and is continued to P. At this point it is connected to the two rods from the diaphragms by a freely moving joint. If one rod pushes with a greater force than the other the hand is moved over to one side, and it will come

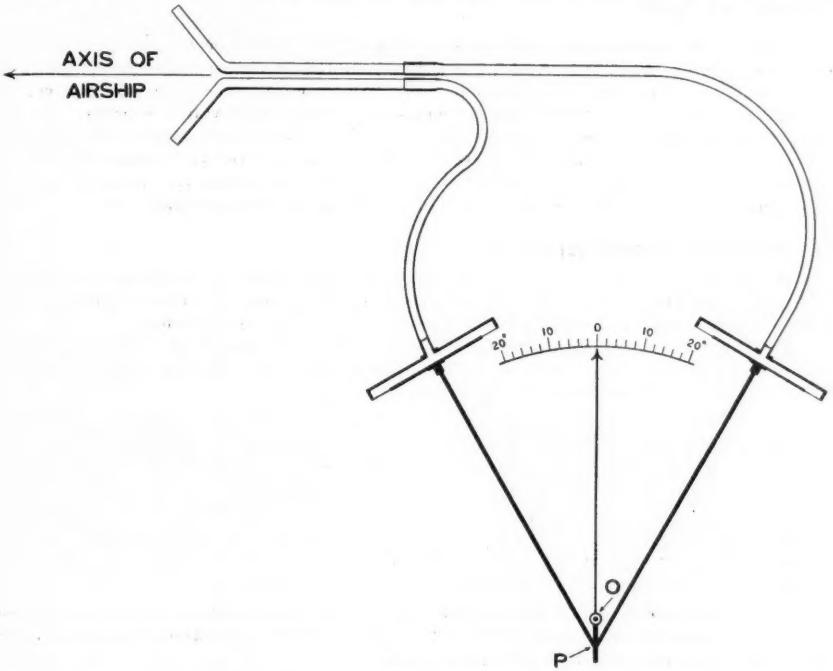



FIG. 5

to rest when OP is in the direction of the resultant of the forces with which the two rods are pushed outwards, and when it is in equilibrium the hand will show on the scale the angle of Yaw. If the speed of the aeroplane increases the hand will not move because the air pressure and consequently the pushing forces in the two rods will both be increased in the same ratio.

The same instrument can be connected to a wind-vane which moves the Y Pitot tubes so as to face the wind. The tubes are arranged to show if the wind has an upward or downward tendency and the angle between the direction of the wind and a horizontal plane is measured.

**Ascending Speed**

I have made an instrument for indicating the speed with which an aeroplane or airship rises or falls. It is roughly made, and at present is only in the experimental stage of development, and it will require remodelling in order to reduce its size and make it more convenient. I will, however, give a short description of it as it may be of some interest.

A clock is arranged to move a valve at equal intervals of time. The valve first connects a vessel to the open air ; it then closes and the air in the vessel is at the same pressure as the air outside. After a short time during which we will suppose that the outside air pressure has fallen owing to the aircraft rising, the valve is again moved so that the vessel is connected to the indicating apparatus. This apparatus measures the excess of pressure of the air in the vessel over the air outside, and gives the change of barometric pressure during the short time between the last two movements of the valve ; that is, it measures the amount of rise in a fixed time or the vertical speed. It does not give the speed at each instant but the average speed during a short interval which terminated a few seconds previously. The indication is a little late and in order to reduce this lateness as much as possible two vessels are used which are opened and closed and connected up alternately to the indicating apparatus.

**Direction and Speed**

Two of the instruments we have considered, the Pitot tube speed-meter and the compass, give us the direction and the velocity of the aeroplane through the air, but unless we know the speed and direction of the wind, we do not know the real direction of flight or the speed over the ground. These instruments give valuable results, but we want to know the direction in which the head of the aeroplane should point in order to get to a definite place, and the speed over the ground as well as through the air. I see no satisfactory solution of this problem, and will not discuss the question, except to suggest that someone here present solves it during the coming year.

**The Principle of Geometrical Design**

We have so far discussed some of the difficulties both in designing and using instruments on aeroplanes. We will now consider the general principles of design and I shall give examples although these have no connection with aeronautics.

I will quote some remarks made by Clerk Maxwell which should be axioms for those who design and make scientific instruments. He wrote, under the heading of "General Principles of the Construction of Apparatus," as follows* :—

> "There are certain primary requisites, however, which are common to all instruments, and which therefore are to be carefully considered in designing or selecting them. The fundamental principle is, that the construction of the instrument should be adapted to the use that is to be made of it, and in particular, that the parts intended to be fixed should not be liable to become displaced ; that those which ought to be movable should not stick fast ; that parts which have to be observed should not be covered up or kept in the dark ; and that pieces intended to have a definite form should not be disfigured by warping, straining, or wearing."

As an instrument maker, I know the difficulty of fulfilling these conditions, and as users of instruments, we all know what our feelings are when a movable part of an instrument sticks fast, and a scale is difficult to read.

Later he says :—

> "Each solid piece of an instrument is intended to be either fixed or movable, and to have a certain definite shape. It is acted on by its own weight, and other forces, but it ought not to be subjected to unnecessary stresses, for these not only diminish its strength, but (what for scientific purposes may be much more injurious) they alter its figure, and may, by their unexpected changes during the course of an experiment, produce disturbance or confusion in the observations we have to make.
>
> "We have, therefore, to consider the methods of relieving the pieces of an instrument from unnecessary strain, of securing for the fixed parts a determinate position, and of ensuring that the movable parts shall move freely, yet without shake.
>
> "This we may do by attending to the well-known fact in kinematics—'*A rigid body has six degrees of freedom.*'

* See Handbook of the Special Loan Collection of Scientific Apparatus in 1876. Introduction by Clerk Maxwell.

"In instruments which are exposed to rough usage it may sometimes be advisable to secure a piece from becoming loose, even at the risk of straining and jamming it; but in apparatus for accurate work it is essential that the bearings of every piece should be properly defined, both in number and in position."

These generalisations cover a wider field than the construction of scientific apparatus, and in some respects apply to all moving machinery; and designs in which this principle is carried out may be called geometrical designs. A three-legged table is a geometrical design, and a four-legged table is not. A four-legged table either rocks on two legs, or bends so that all legs touch the floor, and the amount of bending and the pressure of each foot on the floor depends on the stiffness of the table and the evenness of the floor. Every time an ordinary chair is placed in a new position, it takes a new shape. A surface plate is a familiar example of the importance of three supports, and nearly all scientific instruments rest on three feet. The three-legged table with all its feet touching the floor has some freedom of movement; it can slide in a north and south direction and in an east and west direction, and it can rotate about a vertical axis. If two of its feet only touched the floor, it would have greater freedom of motion, in as much as it can rock also about these two feet, and its freedom would be still further increased if only one foot touched the floor. To completely fix it more must be done. If all the feet touch the floor and one foot be pressed against the wall of the room, that foot can no longer move at right angles to the wall, but it can slide along the wall, and the table can rotate about a vertical axis. If that foot is pressed into the corner of the room all sliding of that foot is prevented and the table can only rotate, and this is prevented by pressing one of the other feet against one of the walls. These six points of contact, three on the floor and three on the walls, prevent all movement, and this definiteness of position could not be given with less than six points of contact. If we want to prevent an object from moving in any direction, it is best to put a fixed stop with its surface square to the direction of movement; this gives the least pressure against the stop. It also causes the least shift of position due to dust lodging between the foot and the stop. In the case of the table just considered this best condition has been fulfilled. If only one of the feet is moved away from the flat surface against which it was pressing, it will be seen that this movement is perpendicular to that surface.

Another example of geometrical design is the method of supporting the trunnions of a transit telescope in two "V" shaped bearings, giving four points of contact. This allows rotation about the axis of the cylinders forming the trunnions and also longitudinal motion along the axis of the cylinder, and a fifth point of contact is required to prevent this last movement. A sixth point of contact would be required to prevent rotation—the only remaining motion possible.

Each case has to be worked out according to the movements required in the piece of mechanism which is being designed. When I speak of points of contact, in reality the contact must be between two surfaces, the area depending on the hardness of the material, the forces between them, and the amount of wear that will take place. If the three-legged table had pointed feet it would run into the floor. In many cases also the contact between the surfaces has to be ensured by pressure from a spring.

## Good Design and Bad Workmanship

A most important consideration in a good design is that the instrument shall still work well when the rubbing surfaces get worn or parts get bent, or if the workmanship is not good. I am not recommending bad workmanship; the workmanship should be good and so should the design. But I do mean that with perfect workmanship and a bad design, you may get jamming in the moving pieces and bending of parts which should not bend, and the results obtained will be liable to error and the working unsatisfactory. This consideration brings out most forcibly the advantage of geometrical designs, but also it is a valuable test of all designs. It is a long way from being the only test, but it is always well worth while to consider separately the effects of imperfect workmanship, or the bending of each part and wearing of the rubbing surfaces. Take the case of wear in a wheelbarrow. The axle of the wheel usually consists of two round iron pins running in holes in wooden rails forming the frame of the wheelbarrow (Fig. 6). Both

the wood and the pins wear; the pin gets smaller but keeps circular, and wears its way into the wood and always fits it properly on the side where pressure is taken. The wheel will work perfectly till either the holes break out of the wood or the pin wears down very small and itself gives way. But sometimes the axle is made differently, an iron rod is fixed to the two wooden rails and passes through a hole bored along the centre of the wheel (Fig. 7). With use the iron rod wears on the under side and does not remain circular the hole in the wheel gets larger; the result is increased friction and a loose and shaky bearing.

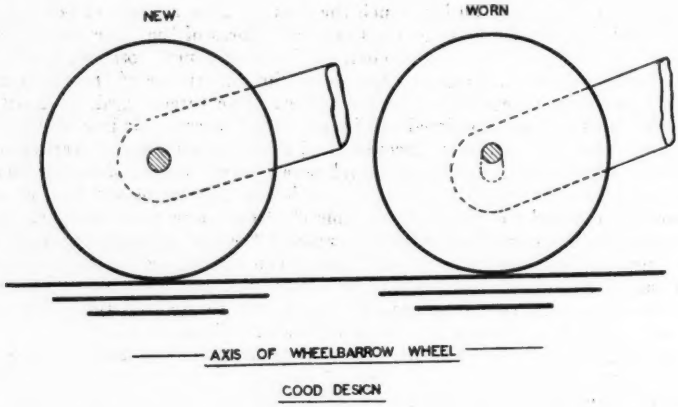



FIG. 6

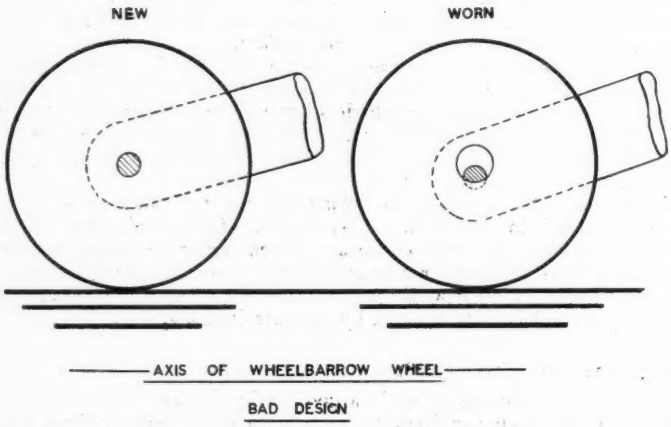



FIG. 7

I have here an instrument, as an example, the Rocking Microtome, which has been designed as far as possible on the geometrical method. It is used for cutting sections of animal tissue for microscopical examination. Sections as thin as 0·003 mm. or 1/8000 in. are cut, and succeeding sections should not vary greatly in thickness. It is clear from this that the instrument must work with considerable precision. The object is embedded in paraffin-wax and moves up and down, the knife, an ordinary razor, being fixed. The rocking arm holding the object to be cut is fed forward between each stroke. I believe this instrument to be a good example of geometrical design. The following test

was applied. The iron castings of which it is chiefly made were taken as they left the foundry and were put together with as little work as possible; it cut good sections at once and my expectations were realised by the results of this severe ordeal. I should add, however, that the screw and some other parts were difficult to make badly and were of the usual form and finish.

It is essential that the rocking arm should rotate with great precision about the same axis every time it rocks. This axis is of an unusual form, and Fig. 8 is a line drawing

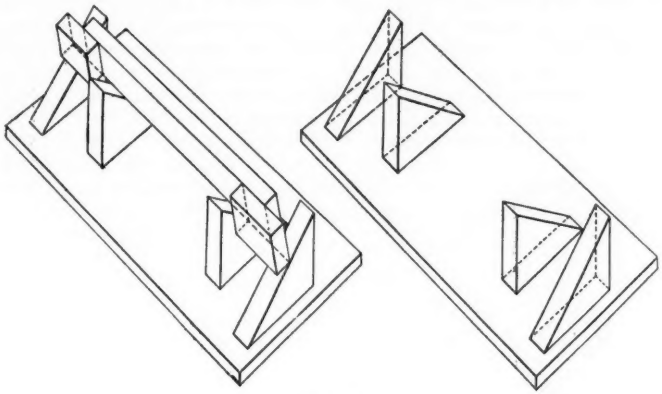


FIG. 8

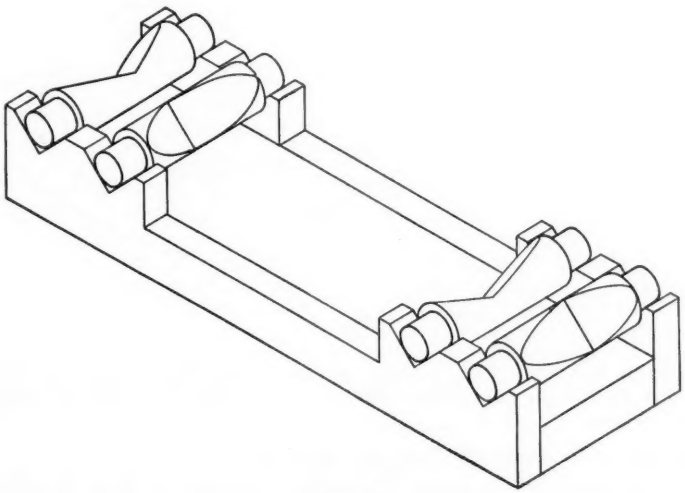


FIG. 9

from a photograph of a wooden model made to illustrate the design. It is a modification of the " V " bearings for trunnions, it is very rigid, and as it is a knife edge bearing it moves with very little friction. There are four knife edges in line; these rest on flat surfaces placed diagonally. In the rocking microtome the knife edges are made of cast iron and have right-angle cutting edges resting on flat cast iron surfaces.

In cases where the rocking movement is small and it is important to reduce the friction as much as possible, another modification can be used with advantage. This is also shown by line drawings of a wooden model (Figs. 9 and 10). Here a hard steel

knife is used, resting on four flat steel faces forming part of round steel rods which rest in V-supports. In this construction each flat face suports a quarter of the load, and the centre of pressure acts approximately at the centre of each flat face. And this takes place even when the knife bends more with increased load on it. Rotation takes place about a definite line in this arrangement also, and it can be used where a knife edge like that in a balance is impossible. It can be roughly made ; it will work well, and is an example of the before-mentioned test for good design.

This test for good design is not the only test, and I will give an example to show how it may fail. Ball bearings are much used, and when once used for any purpose they continue to be used more and more ; this is the best test of a really good mechanical device. All must admire their design, but first-rate workmanship is essential ; in this must be included the composition of the steel, the skill in hardening as well as the accuracy of the figure of the working parts. A ball bearing however would be a better thing even than it is at present if it did not require such fine workmanship. It also requires careful

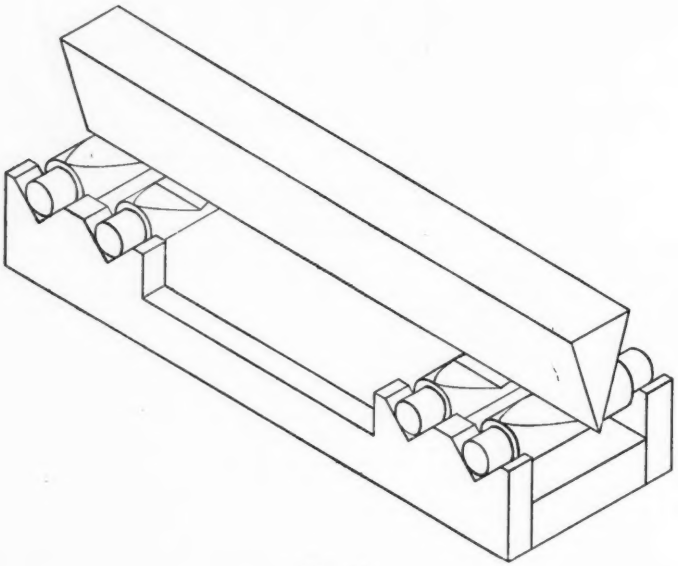


FIG. 10

mounting, and it is interesting to notice that the recent improvements in ball-bearing design are in the direction of allowing it to work satisfactorily on shafting which may be considerably bent.

**The Advantage of Reversing the Parts of a Machine**

An improvement in the design of a machine can often be made by reversing the relative position of two parts of it, or the part that moved can be fixed and the part that was fixed can be made to move. This reversal makes it possible to compare two or more methods, and it is then easy to see which is best. It is advantageous that " the survival of the fittest " should take place early in the life of the machine, and by this means, in fact, it takes place before the design is completed.

In a cutting tool it is a question whether the tool or the work should move : sometimes one is best and sometimes the other. In the wheelbarrow it is easy to see which is the best design, and if the designer had deliberately considered whether the iron pins should turn in the wooden rails or whether the iron bar should be fixed, the bad design would never have been made. It is surprising how often this reversal is possible and

advantageous, and how difficult it is to realise that it is possible. We are so familiar with a clock in which the frame remains at rest and the hands move that it requires a considerable mental wrench to realise that it is possible and in some cases better that the clock itself should revolve and the hour hand remain at rest. But in recording apparatus it is usual to fix the clockwork in the rotating drum carrying the paper, and to prevent rotation of the hour hand spindle. If the clock is at rest, as is usual with clocks, the axis must rotate, but if the axis is fixed the clock itself will be driven round. Another reversal of the usual arrangement is the Gnome engine with its fixed crank and rotating cylinders. I will give one more example of reversing the usual arrangement. In a lathe the tool is usually held rigidly so as to withstand the downward force due to the cut. This force is equal to the upward force on the work, and the tool and the work are held together by the rigidity of the slide rest, the bed, the headstock of the lathe and the work. Now in cutting the thread of a fine screw we find it an advantage to carry the tool on a horizontal axis so that the cutting point is free to move up and down and then to prevent its downward movement by fixing a projecting piece which rests on the top of the work (see Fig. 11). The downward force on the tool is carried to the work by the shortest chain of pieces. The result is the prevention of chatter, good work, and the cutting edge remains sharp for a longer time.

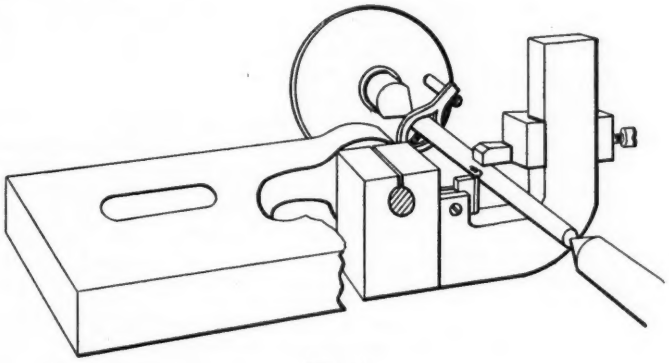


FIG. 11

I have spoken as a manufacturer of scientific instruments, but my remarks apply equally or even more to the home-made or rather laboratory-made type of instruments. And it is with these that the greatest advances in knowledge have been made. If I could believe that what I have said would be any help to the makers of the wire, cork and sealing-wax class of instruments, or to the orthodox instrument maker, I should be glad to think I had done something to advance knowledge.

Wilbur Wright is honoured in this country as he is by our friends and relations across the Atlantic; by our friends and neighbours across the Channel, and indeed by the whole world.

A new chapter in the History of Engineering has been begun. The first page has been written by the man whose name we honour to-night as the pioneer in aviation.

He is gone, but his work will live for all time.

The CHAIRMAN, at the conclusion of the lecture, read the following cable which had been received by Mr. Brewer from Mr. Orville Wright :—" Aëronautical Society's tribute to Wilbur's memory highly appreciated."

The following resolution was then unanimously passed by the meeting for communication to the town of Dayton :—

" The Aëronautical Society of Great Britain tenders its hearty sympathy to the citizens of Dayton in the disastrous flood that has befallen their city, and it hopes that the birthplace of Aviation may soon recover its former prosperity."

## Discussion.

The Chairman : It is not proposed to have a regular discussion on the most interesting lecture we have just heard, but I feel quite unable to express the great admiration, which I feel sure you would wish to express, unless I am assisted. I propose to call to my assistance several gentlemen here who are much more capable than I am, and I will commence by asking Lieut.-Col. Squier, who was with the Wrights for two years working out experiments when the American Government were taking over their first aeroplane, and who is well known in America in connection with the subject of aviation.

Lieut.-Col. G. O. Squier : I feel sure you will join me in proposing a vote of thanks to Mr. Horace Darwin for his most interesting lecture.

It is very fortunate that Mr. Darwin and his company should have turned their attention towards aeronautical instruments, and I feel that the lecturer has contributed substantially to the progress of this new art. It is of first-class importance that we should be able to measure accurately, but measuring on aeroplanes in flight presents problems that are altogether new.

Speaking of Wilbur Wright and of this tribute in his honour, I should like to mention that we have honoured him in our own country by a special vote of thanks of Congress and the striking of a special gold medal which was presented by President Taft at the White House. This is in fact the highest honour we can confer on any individual.

The achievements of Wilbur Wright and his brother are a credit to the Anglo-Saxon people, and therefore I think it is very proper that England and America should join in common recognition of his most valuable work.

I first became acquainted with the Wrights through my association with the American War Department, and got to know them as well as people usually do know each other. They were not easy to get acquainted with.

I would like to refer here to a document prepared at that time, which now has some historic interest. It is hardly believable, but five years ago the first specification for aeroplanes ever contracted for was drawn up in the American War Department in one afternoon, and on a single sheet of paper.

The specification was very simple, and it was issued publicly, as is required by law, on the 23rd of December, 1907.

In order to understand the situation you must remember that the Government had recently granted £10,000 to Professor Langley for experimental work. At that time his accomplishments were not generally appreciated, and the support accorded him was regarded by many as a waste of public money. Consequently the public was in no mood at that time to listen favourably to any proposal to spend still more public money on aircraft.

But the Wright brothers came to the War Department and informed us of what they could do, and they so convinced the authorities that money was found to give them this first contract.

The contract promised to pay £5,000 for any heavier-than-air machine capable of carrying two people weighing in the aggregate 350 lb., with petrol sufficient for 125 miles at a speed of 36 miles an hour. The issue of the contract was criticised severely, and the War Department was supposed to have lost its head.

Coming from the Government, the contract helped the Wrights in France and elsewhere. They constructed a machine to meet the conditions, and finished building it some time in the following September.

Wilbur Wright in the meantime went to France, and so, one brother in America and one in France, they simultaneously carried on their work.

In due course the American machine was delivered to the Government. People heard many rumours about it, but never having seen anyone fly they did not believe in it.

On September 19th Orville Wright went out early in the morning and flew for 58 minutes ; only a few soldiers observed him. The effect was marvellous ; it disorganised Washington completely. That afternoon he flew again at half-past four, in the presence of every man, woman and child who could get up to the aerodrome.

Wilbur Wright kept in close touch with Orville Wright throughout his work. It had been arranged between them that the first flight of an hour in duration should be accomplished in America, and Wilbur Wright arranged his own flights in France so that Orville Wright should make the hour's flight in America first.

A sad accident occurred a few days later, when Lieut. Selfridge was killed, and Orville Wright nearly lost his life. It delayed the contract and disappointed everyone. But Wilbur Wright in France thereupon flew for an hour, mainly I think in order to cheer his brother, who was then lying in the Army Hospital at Fort Meyer, Va.

The American Government extended the time limit for the completion of the contract, and ultimately the Wrights secured a speed bonus so that the total amount paid for the machine was £6,000. The machine with which they made their successful bid is now permanently exhibited in the Smithsonian Institute at Washington, as the property of the American Government.

During the period of the completion of the contract it was interesting to study the character of Wilbur Wright and his brother. They kept their one idea always in sight. Nothing would swerve them from their work. They were there to fulfil this contract.

No one could ever tell when they would fly. Frequently it happened that the President would drive over to see them fly, and almost as often he would have to drive back again disappointed. They were absolutely uninfluenced by any outside forces. Even when Congress came over *en masse*, and they had the entire Government looking at them, they would not fly if they thought it inadvisable. Not only would they not fly, but they would not say why either.

I have never known men with a more wonderful knowledge of the physical properties of materials. They knew the different kinds of wood and metals and how to prepare them. If a part got broken they would sometimes bring a piece of wood all the way from Dayton in order to repair it. Their attention to absolute detail was profound, yet never without a reason.

The brothers were not alike in some ways. They always argued a point, and on any particular question one would take the opposite side to the other. In this way they worked wonderfully well together.

In their contract flight for distance we required them to go five miles and back, from Fort Meyer to Georgetown. It was a very difficult flight and a hard piece of country to cross.

Orville Wright, who had been hurt the year before, insisted on carrying this out, and Wilbur was willing to stand aside, although he could have done it himself.

Progress in the art of flight has rapidly advanced since those days, and it will continue to advance. The use of reliable instruments, about which Mr. Darwin has spoken this evening, will contribute much to that end. I feel the greatest pleasure in asking you to join me in a hearty vote of thanks to the lecturer for his most interesting address.

Col. J. E. CAPPER: I feel it a great honour that I have been asked to second Colonel Squier's motion this evening, an evening notable in the history of the Aëronautical Society.

We have just listened to the first lecture, delivered under the terms of the trust founded by the Society to perpetuate the memory of their late member, Wilbur Wright, and it is to me, who have taken an interest in aeronautics for many years now, a great thing to find that gentlemen like Mr. Darwin, of high scientific attainments in other directions, are turning their attention to aeronautics. It shows how seriously the thing is being taken, when we see leaders of science all over the world devoting their interests to making flying safe. We are really only in the infancy of aeronautics, and I think that those who have done much pioneer work will acknowledge there is much to learn yet.

I would like to say a few words about the gallant gentleman whose memory we are celebrating to-day. I was introduced to the Wright brothers by Mr. Chanute, and I was immensely struck at the way the Wright brothers met me. They knew I was out in America inquiring about flying in 1906, and they told me everything they had done and were going to give me an exhibition flight at Dayton, but the newspapers were inclined to attract too much attention to their work, and they said they regretted that they could not do it, but they showed me their new engine, and we afterwards met as friends in a very quiet way there.

I next met them in this country when they were men of fame. I wrote to Mr. Wilbur Wright to ask if he could see me, although I knew he was as busy as he could be, and he said he was never too busy to see an old friend. This throws, I think, a great light on the character of the man.

I have now very much pleasure in seconding the vote of thanks to Mr. Darwin for his most interesting and instructive lecture.

The CHAIRMAN: I now put to the meeting the vote of thanks to Mr. Darwin, which has been proposed and seconded. Will all in favour please signify in the usual way. (Carried with acclamation.)

Mr. HORACE DARWIN: I thank you most heartily for having passed this vote of thanks, and the proposer and seconder for the very kind way they have spoken of me. I hope that my firm may be able to help to some extent the progress of aeronautics, but I think we all ought to realise what a large amount of enormously important work the National Physical Laboratory is doing in finding out scientific data based on experimental work. I am glad to see the Director is now present. It is largely due to his energy that so much good work has been turned out there, and I am sure you will all agree with me that it is extremely important that this good work should be continued and more data found.

I wish to thank Dr. Glazebrook for letting us have some of the instruments here on the table, and the Royal Aircraft Factory has also been kind enough to supply various things which may be of interest to you. I hope anyone who cares to will come and look at them later in the evening.

I thank you again most cordially for the vote of thanks you have passed and the kind remarks you have made.

Mr. ROGER W. WALLACE: It gives me very great pleasure and I feel it a great honour to be asked to propose a vote of thanks to the Chairman on this occasion. I have known him so far back as our school days, but I am not concerned for the present with his work at school or at college. In his later life he is taking up aeronautics, and I am sure that now he is taking the Chair of the Aeronautical Society we shall see a great advance in the movement of aeronautics, especially from a scientific aspect.

The lecturer has dealt with the first essential in all work of this kind; it must be considered from a scientific point of view. I don't think people in this country know how much scientific work has been done with regard to aeronautics, and I feel sure we shall in future see a development of aeronautics far in advance of anything we have seen up to the present. It is a great pleasure to me to see a scientific instrument maker delivering the first Wilbur Wright Lecture, and this calls to mind another instrument maker of the same nature, Lord Kelvin, the greatest we have had. Although I gave him his first ride in a motor-car and he fully appreciated that movement, yet on my talking of aeronautics he said I was wasting my time. If he were with us now, however, he would, I know, be producing instruments to assist in the movement.

This Society has existed nearly fifty years, and it has kept in touch all the time with the scientific development of aeronautics—hence the connection between Wilbur Wright and this Society.

It was a matter of great interest to me when Mr. Griffith Brewer asked me to meet Wilbur Wright, and when he was here I had the pleasure of going with him in one of the early ascents. I cannot say there has been much change in the apparatus from a scientific point of view, but in all the details there has been a great improvement within the last six years. I remember the launching "gallows" at which you were shocked when you saw it in the first instance. With this we tried the first time, and landed off the rails and stuck on the ground; then we tried again and went 100 yards further, and the third time went up and over some trees. While we were over some trees the engine suddenly stopped, and one could see that there was no opportunity of turning back. I looked at Wilbur Wright, but except that I saw him get a little pale he had absolute control of himself. In this machine there was a lever at the side of both pilot and passenger and a post in the centre, and you could see right down below, for it was of light construction. After a moment or two the engine went on, or we might have fallen in the trees.

A nicer, kinder man than Wilbur Wright it has not been my fortune to meet. We

must remember that it was only by the cool persistence of him and his brother—waiting till the wind suited, risking nothing they could avoid—it was only by this continuous cool and calm work that they were able to give the hour's flight which turned the attention of the whole world to aeronautics.

I hope that in the future the Aeronautical Society will do as they have done in the past, with still larger audiences as the subject becomes of more interest, that they will go on with their scientific work and enlist the interest of the general public in this country and in others.

I now propose a hearty vote of thanks to the Chairman of the Aeronautical Society, and to second this I will call upon Mr. Griffith Brewer.

Mr. GRIFFITH BREWER: The lecturer at the beginning of his lecture expressed a hope that one of these days the method and results of Wilbur Wright's original work should be published. Some time ago the Council of the Aeronautical Society wrote a letter to the Wright Brothers, and suggested that as the Aeronautical Society was the oldest in the world, perhaps it would be the best medium for making that work public. Unfortunately, just after the letter was written, Mr. Wilbur Wright was taken with the illness which terminated in his death. There was a delay, therefore, of a few months before we received a reply. It was in August, 1912, when Mr. Orville Wright, writing to me, replied that in respect to the publication of the tables and other scientific data which Wilbur and he collected in the early years of experimenting, he did not think it would be a good policy for them to publish these until after their Company had secured patent adjudication. At that time it was the possession of this information that kept the Company at the head of aeronautical work in America, and he said that it was the only advantage they possessed over their competitors until they were able to enjoin others by law from the infringement of their patents. Since Mr. Orville Wright sent me this information the Wright Company has won its action in the Courts at Buffalo, and after this decision has been fought in the Supreme Court the objection to publication would apparently become removed. In the letter from the Aeronautical Society it was suggested that we should send a delegate to Dayton to collect that information. I was to be that delegate, and I can assure you, sir, that it does not require a great deal of inducement to take me over to Dayton to spend what I know will be a most interesting time there. I went there in November last year, after these letters, and discussed the publication with him, and although I cannot bind Mr. Orville Wright in any way to give the information to the world, I believe it will be given and also that it will most probably be published through the Aeronautical Society, so I may have yet another seasick passage to the United States to collect and bring back the particulars of the Wright Brothers' early work.

The Chairman referred to a letter I had received from Mr. Orville Wright describing the floods at Dayton, and it may interest you to hear particulars of that terrible disaster; few can realise what an awful thing it was. I may as well read the whole letter, as it is quite a short one:—

"The water covered over half the City. At our Third Street office the water was about ten to twelve feet deep in the street, but did not quite reach the second floor. On Hawthorne Street it was about eight or nine feet deep and stood six feet on the first floor. Most of the things downstairs were ruined. We saved a few of our books and several small pieces of furniture. We might have saved almost everything had we had more notice, but we overslept that morning, and had to be out of the house within one half hour of the time we were up. The water in leaving deposited several inches of a slimy mud over everything. The gutters of the streets of Dayton for hundreds of miles are heaped several feet high with mud, furniture, etc., that has been removed from the residences and stores. In the centre of the City the water was twelve to fifteen feet deep, and the entire stock of many of our merchants was destroyed. Thousands of families have lost everything they had. This is probably the greatest calamity that has ever happened to an American City, as insurance policies do not cover damage by flood.

"During the time of the highest water, fires broke out in the different parts of the city, and nothing whatever could be done to stop them. The large buildings immediately west of our Third Street office burned, and I went to bed the first night of the flood, thinking that my office had burned, and all of the papers and data on aeronautical matters

had gone with it. A constant downpour of rain while the adjoining buildings were burning probably saved the office.

"Our factory was well out of the flood district, but we have not been able to do much work as yet on account of the difficulty in getting our workmen back and forth to work. We have been without regular street car service, electric light and gas for the past four weeks."

Those here—and I believe there are several—who know the Wrights' home, when they think that the water went in all the rooms on the ground floor, will realise what an awful thing it was, and half the town of 300,000 inhabitants was flooded in that way.

I should like to add a few words more of appreciation of the original work which the lecturer has given to us to-night. I think it is better that this first lecture should be on instruments than on, say, the theory of aeroplanes, because all of us must have occasion to use instruments, and we have learnt many things to-night which will be of use to us in our work.

I now have much pleasure in seconding the vote of thanks to the Chairman. (Put and carried with acclamation.)

The CHAIRMAN: I am deeply indebted to you for your hearty vote of thanks, and am very pleased to see Mr. Roger Wallace here as representing the Royal Aero Club. I feel it a great honour to be in the Chair to-night. It is also a great pleasure to be once more associated with my old friend and schoolfellow, Mr. Horace Darwin. I don't recollect everything about school, but I do recollect that the Darwins were always so much in the first flight that no one else had much of a chance. I am sure you are all grateful to Mr. Horace Darwin for his lecture, which appears to be in every way worthy of this memorable occasion. I should like to add my humble opinion that the future of flying is at the present quite inadequately represented and realised. The advent of the fighting aeroplane and the airship will take rank along with gunpowder, and I anticipate a much more extensive sphere of utility for flight and one which will even tend to limit warfare itself. Such swift and direct transport, even if only applicable to passengers and light goods, must before long have an enormous effect in expanding commerce, spreading knowledge, dispersing ignorance, effacing boundaries and lessening national animosities. It may be a far flight to this goal and one which will be attended with much sacrifice. All honour, then, to those who fall by the way! I think we should give all credit and support to those who by their energy, thought and power of investigation tend to lessen these losses and to further this great movement, and I suggest that in no other way can we offer a more fitting tribute to the memory of Wilbur Wright. (Applause.)

The meeting terminated at 10.36 p.m.

# THE MEASUREMENT OF WIND VELOCITY

A Communication to the Research Committee

By B. Melvill Jones, B.A., A.F.Aë.S., and Harris Booth, B.A., A.F.Aë.S.

## The Qualities of a Good Anemometer

1. The essential points of any anemometer are, that it should be correctly calibrated and should read accurately over the whole range through which it is to be used, and that the calibration should not alter with time. The actual calibration of an anemometer by a direct comparison of its readings with its rate of movement through the air is a matter of very great difficulty, and the various methods by which it has been done will be considered later, but when it has once been carried out for one anemometer all others can be easily calibrated from that one by comparison.

2. Besides these obviously essential points there are several qualities which an anemometer should possess, which depend upon the purposes for which it is to be used. For instance, when measuring the velocity of natural winds, in which the direction is rapidly changing, from time to time, it is important that the readings should not be too sensitive to slight variations of wind direction, since it is difficult to construct an instrument to follow these fluctuations very closely; for indoor work in wind channels, &c., this is not a point of much importance.

If, again, the instrument is required to record the actual velocity fluctuations in an unsteady wind the inertia of the moving parts should be as small as possible, but if on the other hand it is the mean velocity that is wanted it is only necessary that the mean reading recorded should be capable of a definite interpretation.

## The Interpretation of the Mean Velocity of a Fluctuating Wind

3. The last requirement raises the rather curious point as to what is to be considered to be the mean velocity of a fluctuating wind, whether it is to be the real mean velocity, or that velocity which would produce a force upon a body equal to the mean force produced by the fluctuating wind. The latter mean velocity will always be higher than the former because the resistance that a body offers to the wind is always proportional to some power of the velocity greater than one.

4. As a general rule the resistance of a body to motion through a comparatively inviscid fluid, such as air, is approximately proportional to the square of the velocity, and therefore the mean wind on the latter interpretation may be taken as equal to the square root of the mean of the squares of the actual velocities.

5. In general it may be taken that the direct velocity mean is required in meteorological work and in navigation, and the mean force producing effect in all other scientific work, and if this generalisation be accepted it follows that the most suitable anemometer for such scientific work will be one that gives a reading proportional to the square of the velocity, since such an instrument will give the mean force producing effect in a fluctuating wind, and the value of this mean will be quite independent of the inertia of the moving parts.

6. If, however, it is required to measure the true velocity mean the readings must be made directly proportional to the velocity, but in this case the presence of inertia will raise the value of the mean, the limits being the true velocity mean when inertia is negligible and the root mean square when the inertia is very great.

7. This question as to whether it is the true velocity of the wind or the force producing effect which is required, arises again when considering anemometers for use in air of varying density, as for instance in the case of an anemometer to be used in an aeroplane where the height above sea level, and consequently the density, is continually altering. If, as is usually the case, the anemometer is a pressure measuring instrument, such as a Dines

tube, the readings will of necessity represent the force producing effect of the wind, and will be greater or less than the true velocity according as the density is less or greater than that for which the anemometer was designed. The difference introduced in this way is of the order of 1·5 per cent. per 1,000 feet.

## Classification of Anemometers

8. The various anemometers in general use can be roughly classified under the following headings :—

(1) Instruments which attempt to measure the actual movement of the air ;
(2) Instruments which measure the force upon some body exposed to the wind ;
(3) Instruments which measure some pressure difference set up by the wind.

9. The most familiar instruments of the first class are the " Robinson Cup Anemometer," and the " Air Meter "(which is a helix suspended with its axis of rotation parallel to the wind direction). Instruments such as these never have a constant calibration coefficient, and are usually very inaccurate at low speeds, but at high speeds these objections do not apply. The friction may be divided into two terms, one constant and the other depending upon the wind force on the moving parts, and therefore upon the square of the velocity. The first term is usually the most important, and is responsible for the inaccuracy at low speeds. The second term has the same proportional effect at all speeds, and is not of much practical importance.

10. The principal advantage possessed by instruments of this type is that they read the actual travel through the air independently of variations in density and hence are suitable for purposes of navigation ; but their uncertainty at low speeds, owing to slight variations in the friction of the moving parts, renders them unsuitable for scientific research work. In a fluctuating wind they would read the true velocity mean if they had no mass, but the presence of inertia in the moving parts always makes the mean reading somewhat high, the actual value being, as previously explained, between the true velocity mean and the mean root square velocity.

11. Anemometers of the second class, that is wind force measuring instruments, consist of a flat plate or other body, placed so that the wind force tends to move it against a controlling force provided either by gravitation or by means of a spring, the difference from the first class being that in a steady wind the moving parts are stationary. Whatever the external form, they may be divided into two distinct sub-divisions, according as the controlling device allows of a deflection proportional to the force on the body, or to its square root. In the former case the deflections are closely proportional to the square of the wind speed, and the mean reading in a fluctuating wind is therefore the root mean square velocity, whatever the inertia of the moving parts may be. In the latter case the deflections are directly proportional to the wind velocity, and the mean reading, as with instruments of the first class, depends on the inertia of the moving parts, and lies somewhere between the true mean velocity and the root mean square.

12. With regard to friction, the same arguments apply as in the first class, except that the actual movement of the parts is usually very small, and therefore the friction can be more nearly eliminated.

13. The principal advantage possessed by these instruments is that they can be made very compact and strong and therefore are particularly suitable for use as pocket anemometers in field work, they are not, however, so suitable for accurate scientific research work as instruments of the third class, and are in fact seldom used for such a purpose.

## Pressure Measuring Instruments

14. Practically all instruments used in accurate scientific research work are of the third class, that is to say they measure some pressure difference set up by the wind ; their principal advantages are, (1) that, since the moving parts may consist of fluids only, friction can be almost entirely eliminated, and, (2) that the parts which have to be in the actual air stream can be made very small.

15. Anemometers of this type must always consist of two distinct parts, one being the device by which the pressure difference is generated and the other the manometer by which it is measured. We shall in this paper confine ourselves to the first of these, but a brief description of some manometers suitable to this class of work will be found in an Appendix.

16. The principle which underlies the action of all instruments of this class is known as Bernouilli's Theorem of Streamline Motion and is as follows :—

Let p be the pressure, v the velocity, and $\rho$ the density at any point in a fluid. Then if the motion be steady and the fluid be inviscid and incompressible

$$p+\tfrac{1}{2}\rho v^2=\text{a Constant}$$

at any point along a streamline. From the above theorem it follows that for conditions under which air behaves as an inviscid incompressible fluid, the pressure in any streamline which has been brought to rest will exceed the pressure in the undisturbed stream by exactly $\frac{1}{2}\rho v^2$, so that if by any means a streamline can be expanded and brought to rest the difference between the pressure within it and the pressure in the undisturbed stream will be a measure of the velocity.

17. It will appear later, when the accuracy of a pitot tube is under consideration, that air may, for this purpose, be considered to be inviscid at all ordinary velocities and incompressible at velocities below about 150 miles per hour.

18. In what follows the pressure in the undisturbed stream will be called the Static Pressure of the stream, the pressure in a stream line which has been brought to rest will be called the "Bernouilli Constant," whilst the difference between the Bernouilli Constant and the Static Pressure will be called the Dynamic Pressure.

19. Bernouilli's equation may now be written :—

Static Pressure+Dynamic Pressure=Bernouilli Constant.

Or since the Dynamic Pressure is equal to $\frac{1}{2}\rho v^2$.

$$v^2 = \frac{2}{\rho} \quad \text{[Bernouilli Constant—Static Pressure.]}$$

**The Pitot Tube**

20. In practice the Bernouilli constant of a wind can be obtained with the greatest ease ; all that is necessary being to place a tube having one end closed and the other open in such a position that the open end faces the wind. All modern experiments agree that the pressure inside this tube will equal the Bernouilli constant of the wind with great accuracy whatever the diameter of the tube, within very wide limits ; tubes with diameters varying from $\frac{1}{10}$ to 4 or 5 inches have been actually tried, but the law is probably correct over a much wider range. Modern experiments further show that the accuracy with which such a tube reads the Bernouilli constant is practically independent of the cross section, the shape of the walls at the mouth, or the coincidence of the axis with the wind direction within about 20 degrees on either side. A tube of this nature is generally called a pitot tube, and from what has been said it will be seen that anything that looks like a tube pointing approximately into the wind is a true pitot tube, no accurate measurement or setting up being necessary to ensure that the pressure shall be accurately equal to the Bernouilli constant.

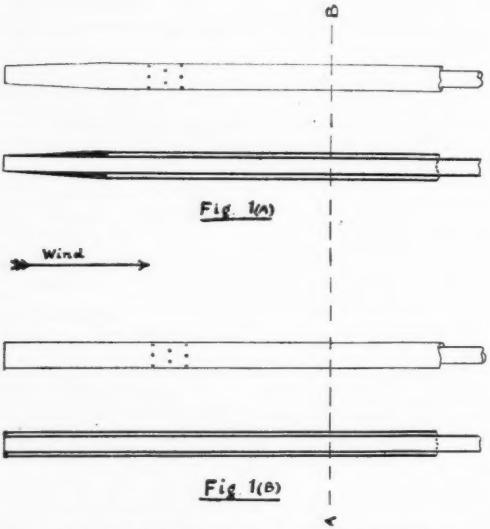
21. The problem of measuring wind velocity by the pressure difference method resolves itself therefore into one of obtaining the static, or some equivalent pressure to be subtracted from the Bernouilli constant, and the various methods of measuring wind velocity in common use differ only in the way in which this lower pressure is obtained.

**The Static Pressure Tube**

22. This, as its name implies, is a tube by which the actual static pressure of the wind can be obtained ; it consists simply of a uniform tube enclosed at both ends, but with a series of small holes, evenly distributed around a circumference, at some distance from either end. If this tube is placed with its axis along the wind direction the pressure

within it will equal the static pressure of the wind with considerable accuracy. If the axis of this tube does not make an angle of more than about 5 degrees with the wind direction and if the circumferential holes are not less than 6 diameters away from either end and have no external burrs or roughnesses upon them, the accuracy with which the pressure within will equal the true static pressure of the stream will be considerably greater than $\frac{1}{2}$ per cent. of the dynamic pressure of the stream. If, however, any of these conditions are not complied with the pressure will probably show a tendency to fall below the true static pressure.

23. A combination of pitot and static pressure tube as above described forms an ideal wind measuring device when the wind direction is known and is fairly steady, for since the pressure difference can be relied upon to approximate very closely to the value $\frac{1}{2}\rho v^2$ the instrument requires no experimental calibration, provided only that the static pressure tube conforms to the conditions above enumerated and that there are no leaks in the tubes. The latter condition is an important one because a leak usually appears as an alteration in the calibration and gives no indication of its presence since it does not destroy the apparent consistency of the readings.




FIGS. 1(A) AND 1(B).—TWO FORMS OF COMBINED PITOT AND STATIC PRESSURE TUBES (IN ELEVATION AND SECTION)

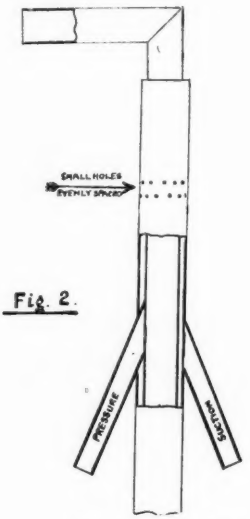



FIG. 2.—DINES TUBE

24. This form of instrument is very extensively used in scientific research work. In the earlier forms the pitot and static pressure tubes were mounted separately, but in more modern forms the pitot tube is placed within the static pressure tube as shown in Figs. 1, A and B, which are scale drawings of the latest types of this instrument.

25. In Fig. 1A is shown a side elevation and a section of the standard instrument used at the National Physical Laboratory, and is in fact a drawing of the actual instrument upon which most of the experiments described in paragraph 40 were performed. Fig. 1B shows a form which is considerably easier to construct but which gives equally accurate results.

26. The constructional details which require particular attention are as follows. The static pressure holes should have no external burrs and should be no nearer to the open end than is shown in the figures, and any bends or attachments upon the outside of the instrument should lie behind the line AB. If the clamp holding the tubes is large it is safer to place it considerably behind AB.

**The Dines Tube**

27. It has been stated that for a static pressure tube to read correctly its axis must coincide with the wind direction within about 5 degrees, and for outdoor work in which the direction of the wind is prone to sudden and wide fluctuations from the mean direction this is a serious objection. The difficulty can, however, be overcome by using a modified form due originally to Mr. Dines, F.R.S.

28. In this instrument, generally called a Dines tube, the static pressure, instead of having its axis parallel to the wind direction has it perpendicular thereto, so that fluctuations of wind direction in a plane perpendicular to the axis do not affect the readings at all, provided that the holes are symmetrically placed round the tube. The pressure within this tube does not, however, equal static pressure of the air, but is lower than this value by an amount which may vary between about $0{\cdot}1\rho v^2$ and $0{\cdot}35\rho v^2$, so that the total pressure difference between the two tubes will be somewhere between $0{\cdot}6\rho v^2$ and $0{\cdot}85\rho v^2$.

29. The actual value of the pressure to be found in the low pressure tube is very sensitive to minute changes in the shape of the circumferential holes and even to the formation of the interior of the tube, so that every Dines tube, unlike the pitot and static pressure combination, requires individual calibration, its sole advantage over the latter type being that its readings are independent of wind direction to about 20 degrees on each side of the mean.

30. Dines tubes are extensively used for meteorological work, the latest type being of the form shown in the scale drawing, Fig. 2.

31. As in the case of the pitot and static pressure tube the earlier forms had the two tubes mounted separately, the alteration being merely one of convenience and not of principle.

32. For meteorological work the axis of the low pressure tube should of course be placed vertical since the wind near the ground fluctuates principally in a horizontal plane.

**Measurement of the Rate of Flow in Channels and Pipes**

33. When the fluid of which the velocity is required is confined in some way, it is often possible to measure the velocity by somewhat modified methods. If for instance the flow is along a pipe or channel with parallel sides, the static pressure tube may be replaced by a hole in the side of the pipe in the cross section at which the velocity is required, since the static pressure over a cross section of a parallel channel is known experimentally to be very nearly constant.

34. With this arrangement the pressure difference between the pitot and the hole in the side will be equal to $\frac{1}{2}\rho v^2$ where v is the velocity (*at the pitot*). Such a method is particularly applicable to long pipes of small diameter, since it is a matter of considerable mechanical difficulty to make a very small static pressure tube. In this method it is of the utmost importance that the hole in the side should be perfectly free from all burrs and other irregularities.

35. If the velocity is required in the central region of a large pipe or channel within two or three diameters of the intake, the pitot tube can also be dispensed with and replaced by the pressure in the undisturbed fluid outside the channel, the assumption in this case being that no energy loss occurs in the central streamlines from the still air outside the channel to the place at which the velocity is required.

36. Experiments which have been conducted upon this point show that the above assumption is very accurate for several diameters from the intake, it being practically impossible to detect any difference between the pressure in the undisturbed air and that in a pitot tube in the central region within two or three diameters of the mouth.

37. The latter method is the one used to determine the velocity in the four-foot experimental channel at the National Physical Laboratory, but in this case the point at which the velocity is required is some distance within the channel and the intake is somewhat obstructed by a honeycomb, so that a considerable loss of energy occurs between the outside air and the point of experiment, and it is therefore necessary to calibrate the arrangement. This is done by comparing the readings of a standard pitot and static pressure tube placed at the point of experiment, with the suction observed in a hole in

the side relative to the pressure of the still air in the room. When experiments are being performed the pitot and static pressure tube is removed, leaving the flow in the channel entirely undisturbed by any measuring device except a small hole in one of the sides.

38. The method used by M. Eiffel in his laboratory at the Champs de Mars, may be called a further modification of the above, although it actually antedated it. In this case the sides of the channel were entirely removed for a short distance inside an hermetically sealed experimental chamber, so that the static pressure of the stream was simply the pressure of the experimental chamber and the velocity could be inferred from the pressure difference between the experimental chamber and room in which the intake was situated.

**Experimental Evidence upon the Accuracy of the Pitot and Static Pressure Tube**

39. It has been stated that the pressure difference set up by a properly designed "pitot" and "static pressure tube" combination is very accurately equal to $\frac{1}{2}\rho v^2$. It is now proposed briefly to consider the evidence upon which this statement was founded.

40. The following are some of the latest and most accurate experiments which have been performed on this subject:—

1. At the National Physical Laboratory, instruments, both of the concentric and of the separate pitot and static pressure tube type, have been very exhaustively calibrated upon the whirling arm at speeds ranging from about 14 to 53 feet per second.

The radius of the arm is 30 feet.

2. An instrument of the separate tube type was calibrated in water in a circular whirling tub at a radius of only 2 feet, the highest velocity reached being about 2 feet per second.

3. In Germany a concentric type instrument almost exactly similar to that used in experiment (1) was calibrated upon the dirigible Schwaben by direct comparison with the time taken to cover a given distance, flights being made several times in both directions and the range of speed experimented upon being from about 37 to 64 feet per second.

4. In M. Eiffel's laboratory at the Champs de Mars in the discontinuous experimental channel already mentioned, the difference in pressure between a common pitot and the stationary air in the experimental chamber was compared with the readings of an instrument described as a venturi-meter, which had been previously "calibrated by direct measurement" upon a French army experimental biplane.

The speed of calibration was not given, but was probably in the neighbourhood of 50 feet per second.

41. In none of the experiments above quoted did the difference of pressure generated depart from the theoretical value of $\frac{1}{2}\rho v^2$ by a measurable amount, and in experiments 1 and 2 at least the limits of possible experimental error were considerably less than $\frac{1}{2}$ per cent.

42. Considering the experiments in order it can be seen from No. (1) that up to a speed of 53 feet per second a correctly constructed instrument of the above type will give the theoretical pressure difference with great accuracy when travelling in a circle of small curvature.

43. Experiment No. (2) is of interest in that it shows that a very severe curvature of the path does not materially affect the readings, so that it can be directly inferred that the comparatively small curvature of the path in experiment (1) will not render the calibration inapplicable to a straight line path. There is also the point which is of minor interest to the present investigation, that the laws hold equally well for water as for air.

44. Experiment No. (3) is mainly of interest in that it extends the range over which the experimental calibration has been carried, and because it provides a convincing proof that small differences in construction do not affect the readings of this particular type of instrument, the experiments having been carried out quite independently.

45. Experiment No. (4) is mainly interesting because it proves that the pressure in the pitot is really equal to the static pressure plus $\frac{1}{2}\rho v^2$, and hence that the nominal static pressure tube of the combined instrument reads the true static pressure of the air. Experiments (1), (2) and (3) of course merely proved that the difference in pressure between the tubes was $\frac{1}{2}\rho v^2$.

**Theoretical Considerations upon the probable range of accuracy of the Pitot and Static Pressure Tube**

46. The aforementioned experiments only carry the pitot and static pressure tube calibration up to a velocity of 64 feet per second, and in the absence of further experimental work on the subject it becomes important to consider on theoretical grounds up to what speeds the theoretical pressure of $\frac{1}{2}\rho v^2$ represents the true pressure with sufficient accuracy.

47. In the first place, theory says that the pressure in the pitot is exactly equal to $\frac{1}{2}\rho v^2$ in an inviscid incompressible fluid, and in the present state of our knowledge of the nature of fluids, viscosity and compressibility are the only physical properties of the fluid which could upset this law. Let us consider these two disturbing factors separately. The viscous forces within a fluid are proportional to the rate of shear at the point under consideration, and hence, other things being equal, to the velocity at which the motion is taking place. The pure pressures are, on the other hand, proportional to the square of the velocity, and consequently the relative importance of the viscous forces must become less and less as the velocity increases. It seems, therefore, practically certain that since it can be shown that the pressure in a pitot at a certain speed coincides with the theoretical pressure for an inviscid fluid, the viscosity will have no power to upset the coincidence at higher velocities.

48. With regard to the influence of compressibility upon the accuracy of the pitot pressure, Lord Rayleigh has shown that the theoretical pressure in a pitot in a compressible fluid is to a first approximation equal to $\frac{1}{2}\rho v^2 \left[1+\frac{1}{4}\frac{v^2}{V^2}\right]$ where V is the velocity of sound in the fluid. The velocity of sound in air is in the neighbourhood of 1,100 feet per second, hence it follows that unless the velocity of the air exceeds about 150 feet per second the error due to compressibility cannot exceed $\frac{1}{2}$ per cent., whilst if a 1 per cent. error is allowed 200 feet per second is permissible.

49. From the above considerations, then, it would seem to be perfectly safe to predict that readings as obtained from a standard pitot tube will have the theoretical value of $\frac{1}{2}\rho v^2$ within $\frac{1}{2}$ per cent. in air, provided that the velocity is not greater than 150 feet per second.

## Appendix I

**The Value of $\rho$ for Air**

It has been stated that the calibration of the combined pitot and static pressure tube is given by the equation—

$$\text{Difference of pressure} = \tfrac{1}{2}\rho v^2.$$

In this connection it should be noted that if $v$ is the velocity of the fluid in feet per second and $\rho$ the density in pounds per cubic foot, the difference of pressure given by the above equation will be in *poundals* per square foot, and must be divided by "g" to give pounds per square foot. To avoid this inconvenience it is usual to express the density of the fluid as the density in pounds per cubic foot divided by "g." This for air at normal temperature and pressure is 0·00238. If this value be used for $\rho$ the equation becomes—

$$\text{Difference of pressure} = 0{\cdot}00119 v^2.$$

With this equation the difference of pressure will be in pounds per square foot if $v$ is measured in feet per second.

APPENDIX II

## Manometers used in the Measurement of Wind Velocity

The pressure difference between a pitot and static pressure tube in a wind of 20 feet per second is about one-tenth of an inch of water, and if it is required to measure such a pressure to an accuracy of ½ per cent., it becomes necessary to use some form of manometer which is sensitive to 1/2000 of an inch of water.

The form of manometer in use for this purpose at the National Physical Laboratory is known as an oil and water tilting gauge, and consists of a U-tube of a somewhat specialised form mounted upon a frame which can be tilted by means of a graduated screw. The pitot and static pressure tubes are connected to opposite limbs of this tube, and the consequent movement of the fluid in the tube is neutralised by tilting the frame until the position of the fluid returns to its zero position. The equivalent head of water is then calculated from the amount of tilt that has been required. The position of the water in the tube is observed by means of a microscope focussed upon the surface of separation between the water in the tube and a quantity of castor oil introduced into the connecting branch between the two limbs of the U.

A description of such a tilting gauge will be found in the Minutes of Proceedings of the Institute of Civil Engineers, Vol. clvi. Session 1903–1904, Part I., in a paper by Dr. Stanton on "The Resistance of Plane Surfaces in a Uniform Current of Air."

If the accuracy required is not greater than about 1/500th of an inch the oil film can be dispensed with and the microscope focussed direct upon the water miniscus in one of the limbs of the U-tube.

At the Aeronautical Laboratory at the Champs de Mars M. Eiffel used for the same purpose an "Inclined Alcohol Manometer," the alcohol being contained in a graduated U-tube which is inclined to give greater sensitivity. Such a manometer is not so sensitive as an oil and water tilting gauge, but would probably be sufficiently sensitive for use with somewhat higher air speeds.

Manometers of the types described above would of course be quite unsuitable for use upon a moving machine such as an aeroplane, but since the speed of a modern aeroplane is seldom below 60 feet per second, corresponding to about 1 inch of water, very great sensitivity is not required.

A manometer used in connection with the measurement of wind velocity upon the aeroplanes at the Royal Aircraft Factory is in the form of a two-fluid concentric U-tube. This is in effect a U-tube having one limb of the U inside, and concentric with, the other, so that the readings are unaffected by tilting the whole apparatus.

The two fluids must be such that they will not mix and should have their relative densities such that the movement of the surface of separation will be considerably greater than if water alone were used.

## LIST OF FATALITIES

| Date. | Name. | Nationality. | Machine. | Place. |
|---|---|---|---|---|
| May 27. | Lieut. Desmond Arthur .. | Irish | B.E. 205 (2) | Montrose. |
| June 29. | R. Wight .. .. .. | English | Avro (2) | Shoreham. |

(1) Monoplane. (2) Biplane.

# NEW BOOKS AND PUBLICATIONS

*The Curtiss Aviation Book. Glenn H. Curtiss and Augustus Post. London: Grant Richards. 1913. pp. 307. Illus. Price 5s. n.

*With the Airmen. Claude Grahame-White and Harry Harper. London: Henry Frowde, Hodder and Stoughton. 1913. pp. 283. Illus. Price 6s. n.

Molesworth's Pocket Book of Engineering Formulæ (27th Edition.) London: E. and F. N. Spon, Ltd. 1913. pp. 936. Figs.

Meteorologische Ausbildung des Fliegers. Dr. Franz Linke. München: R. Oldenbourg. 1913. pp. 78. Figs. M. 1.70.

Das Schadensersatzrecht der Luftfahrt nach geltendem Recht und de lege ferenda. Dr. Alex. Meyer. Frankfurt a. M: Gebrüder Knauer, 1913. pp. 116. Price M. 2.50

Essais et Réglage des Moteurs. (2nd Edition.) G. Lumet. Paris: H. Dunod et Pinat. 1913. pp. 212. Figs. Price 5 francs 50.

L'Aviation. (6th Edition.) Paul Painlevé, Emile Borel and Ch. Maurain. Paris: Librairie Félix Alcan. 1913. pp. 297. Figs. Price 3 francs 50.

Traité Pratique du Moteur Gnome. André Preynat. Paris: H. Dunod et E. Pinat. 1913. pp. 55. Figs. Price 2 francs 50.

Causeries Techniques sans formules sur l'Aeroplane. Capt. du Genie *Duchêne*. Paris: Librairie Aeronautique. 1913. pp. 258. Figs. Price 6 francs.

Le Gaz Pauvre est-il réellement avantageux? (2nd Edition.) Institut Scientifique et Industriel. Paris: Bibliothèque Pratique du Mois Scientifique et Industriel. 1913. pp. 79. Figs. Price 2 francs 75.

Guide de l'Aviateur. Roland Garros. Paris: Pierre Lafitte et Cie. 1913. pp. 95. Illus. Price 1 franc 50.

Die Wahrheit über den Stand der Luftschiffahrt 1913. Victor Silberer. Wien: Verlag der "Allgemeinen Sport-Zeitung." 1913. pp. 71.

Hilfsbuch für den Flugzeugbau. Dipl. Ing. O. L. Skopik. Berlin: Richard Carl Schmidt. 1913. pp. 212. Figs. Price M. 6.

Hurricanes of the West Indies. Oliver L. Fassig. Washington Government. 1913. pp. 28. Plates. Price $1.50.

Avionnerie Militaire. C. Ader. Paris: Berger-Levrault. 1912. pp. 104. Figs. Price 3 francs.

The Year Book of Wireless Telegraphy and Telephony. London: The St. Catherine Press. 1913. pp. 563. Illus. Price 2s. 6d. net.

Flying and Some of its Mysteries. V. E. Johnson. London: Henry Frowde, Hodder and Stoughton. 1912. pp. 138. Figs. Price 1s. 6d.

Bulletin de l'Institut Aerotechnique de l'Université de Paris. Fascicule III. Paris: H. Dunod et E. Pinat. 1913. pp. 134. Figs. Price 6 francs.

L'Ornithoptère. F. M. Paris: Asselin et Houzeau. 1913. pp. 104. Figs.

Reorganisation and Costings. H. Preen. London: Simpkin, Marshall, Hamilton, Kent and Co. 1913. pp. 188. 2s. 6d.

Aviation. Claude Grahame-White. London: Collins, Sons and Co. 1913. pp. 283. 1s. n.

*The Resistance of the Air and Aviation. G. Eiffel. Translated by Jerome C. Hunsaker. London: Constable and Co. 1913. pp. 242. Figs. Price 42s.

The Airman's Vade-Mecum. No. 1: Meteorology. Col. H. E. Rawson. London: "Aeronautics." 1913. pp. 65. Illus. Price 1s. 6d.

Elementary Practical Mathematics. Prof. J. Perry. London: Macmillan and Co. 1913. pp. 232. Figs. Price 6s.

Les Cerfs-Volants Observatoires. C. Romain. Paris: Librairie Militaire Berger-Levrault. 1913. pp. 30. Figs. Price 75 centimes.

## PAMPHLETS

The Winds in the Free Air. Charles J. P. Cave. Royal Institution of Great Britain. 1913. pp. 10.

Wind Barometer Table. Prof. E. B. Garriott. U.S. Department of Agriculture. 1913.

The Construction of a Weather Bureau Kite. A. J. Henry. Reprinted from Bulletin of Mount Weather Observatory. 1909.

The Influence of the Atmosphere on our Health and Comfort in Confined and Crowded Places. Leonard Hill, Martin Flack, James McIntosh, R. A. Rowlands, H. B. Walker. Washington: Smithsonian Institution. 1913. pp. 96.

Advisory Committee on the Langley Aerodynamical Laboratory. Washington: Smithsonian Institution. 1913. pp. 5.

* Reviewed in this issue.

# REVIEWS

**As Experiencias Aerostaticas de Bartholomeu Lourenço de Gusmao.** By Gustavo Tedeschi Corrêa Neves, Capitao de Artilharia. (Lisbon: Aero Club of Portugal. 1913. pp. 55.)

THIS little brochure is a reprint from the Journal of the Aero Club of Portugal. Its object is to commemorate the bi-centenary of Lourenço de Gusmao, who is claimed to have made experiments with air balloons in 1709, that is, 74 years before the attempts of Montgolfier in France.

Lourenço is said to have succeeded in constructing an apparatus which transported him a kilometer. Unfortunately no accurate description of the balloon has yet been unearthed. It is now generally acknowledged that the drawing and description of the Passarola, published in 1784, cannot possibly represent the actual design of the "machina aerostatica," for, if it did indicate the apparatus, then Lourenço certainly never made his flight.

Lourenço certainly made serious experiments, but the measure of his success is uncertain. The fruits of his labour perished with him. He had neither teacher nor pupil nor assistant.

The study, therefore, of the claims of the Portuguese-Brazilian inventor—for, born in Santos, he came to Lisbon University—becomes practically a purely literary problem. As such it is full of interest. Captain Gustavo Tedeschi Corrêa Neves has evidently made a patient, critical, and comprehensive study of all the documents. Many of these documents were in manuscript, and it is just possible that further information may hereafter come to hand.

Perhaps the most interesting document is the petition to the King by Lourenço seeking licence to construct his machine. He promises, *inter alia*, that his apparatus would travel 200 leagues and more per day; that strongholds could be reinforced and revictualled, and persons brought away without fear of attack by the foe; that the Portuguese might have the glory of discovering the lands round the Pole; and "other infinite advantages which time will show."

His Majesty's disappointment at the solitary one-kilometer flight is suggested as the reason for Lourenço's precipitate journey to Spain. On his way to Madrid he was stricken with fever and died.

This brochure should certainly be translated into French or English, for it contains subject matter which should interest a fairly wide circle of students.

J. E. H.

**Bulletin de l'Institut Aerodynamique de Koutchino.** (Fascicule IV.) (Moscou: Imprimerie I. N. Kouchnereff & Cie. 1912. pp. 140. Illus.)

THE latest Bulletin from l'Institut Aerodynamique de Koutchino deals largely with corrections, not only those which have been found necessary in their wind-tunnel but also with ones that should be taken into account when comparing the results of other experimenters. In the Riabouchinsky tunnel, owing to an incorrect calibration of the anemometer, the wind velocities were always read with too high a value, and the air pressure results obtained in previous Bulletins must therefore all be reduced by 3 per cent. to obtain the correct figures.

Riabouchinsky has carefully studied, with the aid of smoke streamlines, the air-flow through a model Eiffel tunnel. He arrives at the conclusion that there probably is a diverging and not a parallel flow, as was previously pointed out in these columns in a review* of Eiffel's monumental work. His discussion on this point with M. Eiffel is here reproduced from *L'Aerophile*. Two other notable points have been investigated. Riabouchinsky does not believe in the unstable position of the centre of pressure between the angles of approximately 20° and 40°. He, therefore, very carefully tested the planes similar to those experimented upon by Rateau, and finds that the movement of the centre of pressure and the pressure itself is a continuous function of the angle of incidence of the planes. He attributes Rateau's results to incorrect balancing.

The volume concludes with an interesting description of the new hydrodynamical laboratory.

Several of the papers are reprints from the pages of *L'Aerophile*. Now that they are collected together they will be re-read and compared, for there is much good work in them.

F. H. P.

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* AËRONAUTICAL JOURNAL, July 1911.

**Les Aeromobiles.** By R. Goldschmidt. Preface by Ernest Solvay. (Paris: H. Dunod et E. Pinat. 1911. pp. 417. Illus.)

THE author explains that in adding another volume to the already large number of works on aeronautics his idea is not only to form a *résumé* of the fundamental principles and progress of aerial locomotion, but also to combat certain erroneous ideas concerning dirigibles—these repeated in various works to the extent of being taken as fundamental have seriously retarded their progress—the outcome being that dirigibles have grown in size until they have become unmanageable except in fine weather.

In the chapter entitled "Le Sphéro-Dirigeable" he describes the type of lighter than air aeromobile which the author considers the best solution of the problem of aerial navigation until a really reliable heavier than air machine has been evolved. The author created the dirigibles Belgique I., II., III., in collaboration with M. Solvay, M. Godard and M. Vivinus, also M. Vreedenburgh constructed for him the monoplane of that name.

Now that interest in dirigibles has been revived I would strongly recommend the study of this well-written and illustrated book, especially the chapter dealing with the author's plea for a much shorter type of dirigible. There is no doubt that the long type as represented by the Zeppelins has serious drawbacks, in that if from any cause, as in landing, the long delicate framework touches any obstacle it then acts as a girder and the whole edifice crumples up like a house of cards, the extreme length renders it at once unmanageable unless it is kept head on to even a moderate wind, and the added great expense in building, equipping and running one of these monsters militates against their practical use, whereas several compact, serviceable and more efficient machines would be built for the same money—also the great expense for hangars would be considerably reduced. Our Admiralty has been well advised in not building any more long type dirigibles after the Barrow fiasco, and the War Office should stick to their short type but with more powerful engines.

In the second part the author gives six chapters to aeroplanes and examples, the most interesting being those showing the evolution of the Bleriot monoplane.

Chapter 7 treats on the means of developing Aerial Navigation, and, if somewhat inadequate, is nevertheless suggestive.

G. P. D. S.

**Les Aviettes: etude et historique de l'aviation sans moteur.** By G. Houard. (Paris: *La Revue du Cerf-Volant.* 1913. Illus. pp. 30. 1 franc.)

M. HOUARD, in giving us the first chapter in the history of the "aviette"—an untranslatable word that we shall have to anglicise—has no axe to grind. He tells the story as a critical expert—and a very competent one—and the result is all that could be desired. The aviette has not yet reached this country—at least not officially, but an opinion may be hazarded that if some scientific or sporting philanthropist would put up a prize for the purpose of its development, there would be quite a large number of entries and some very astonishing performances. In the aviette lies the germ of a flying-machine which has yet to come into its own—the ornithopter.

It is difficult to see a future for the present idea of motorless flight: a bicycle decorated with fixed planes and a pedal-driven propeller; but one can visualise a man essaying an erratic tortuous flight with flapping-wings. One can go so far as to say that it has been done, and recently, in this country, and it can be done again.

T. O'B. H.

**Le Moteur à Explosion.** By Capt. Martinot-Lagarde. (Paris: Berger-Levrault. 1912. pp. 291. Figs. Price 5 francs.)

THIS is an attractive little book of an eminently useful character. The author has set himself the task of meeting the requirements of the practical man who uses and is genuinely interested in petrol engines, but has not the time or the necessity to become an expert. In this really difficult undertaking he has succeeded quite admirably. The book is divided roughly into three parts. The first is devoted to a very simple but sound exposition of just that modicum of theory necessary for any clear idea of the internal happenings in a motor. This is quite short, and the reader is permitted to escape the eminently boring chapter labelled "Historical" which is usually tacked on to this portion.

The second part, forming the bulk of the volume, deals with the main organs of the motor and the laws governing their successful working. The information given here is commendably up to date and the drawings and illustrations are beautifully clear; the chapter on carburation in particular being most interesting and instructive.

The third part gives the reader some simple instructions as to the running of engines and the various adjustments that have to be made to keep them in good order. Two

useful chapters are devoted to the troubles to which motors are liable and the methods of diagnosing them.

The whole book is written in an easy, interesting style, the theoretical parts calling for no mental gymnastics and being agreeably seasoned with references to results obtained in modern practice. It can be recommended without hesitation to the class of reader for whom it is intended—a class certainly including aviators.

Considering the general soundness of the work it is a little surprising to detect the author in an obvious error on the point of the relative size of exhaust and inlet valves demanded by theory. His claim that the exhaust should be larger than the inlet is clearly incorrect, the exact opposite being indicated.

G. F. M.

**On the Positions assumed by Birds in Flight.** By Bentley Beetham, F.Z.S. (From the Smithsonian Report for 1911, pages 433—439). (Washington Government Printing Office, 1912.)

THIS little pamphlet on Bird Flight is a most welcome addition by Mr. Bentley Beetham to the already very excellent work which has been given us by Mr. Headley. There are a very good series of photographs showing the way in which a gannet starts to fly from a cliff and also alights. The splendid way in which these birds soar and fly in the gustiest of weather make them worthy of the study of all those interested in aviation, and to such this short monograph on their flying gives a most excellent and instructive description of their capabilities.

F. H. P.

**The Aviation Pocket-Book for 1913.** By R. Borlase Matthews. (London: Crosby Lockwood and Son. 1913. pp. 176. Illus. Price 3s. 6d.)

THE author prefaces this handy pocket book by stating that having been in intimate touch with the progress of Aviation since the early days of Wilbur Wright's first actual flights, he had accumulated a mass of useful data which he has arranged in this concise form by eliminating, as far as possible, mathematical matters and information contained in similar books—so as to form a guide or companion to standard works.

Originally it was commenced jointly with Mr. W. B. Esson, Past President of the Society of Civil and Mechanical Engineers, but pressure of work prevented this arrangement being carried out. He also acknowledges the services of Mr. L. Preston Parker, B.Sc.

The matter is split up into ten division:—

No. 1.—Treating of air pressures and resistances concisely and up to dafe.

No. 2.—The elementary theory and design of aeroplanes. Here he quite correctly points out that a complete and practical theory of value to designers has not been yet formulated and this subject is only therefore capable of general treatment, but on the designing or constructing of machines he does not touch; a subject on which reliable information is much in request.

No. 3.—Structural materials is more fully treated and would form the basis of most useful matter if brought up-to-date by collaborating with a practical constructor.

No. 4.—Engines. This has useful comparative tables and ratings.

No. 5.—Example of actual machines. A selection of such is always an invidious matter, as however carefully compiled some one considers himself left out, but it certainly should be brought up to date if it is to be of practical use.

No. 6.—Piloting and Aerial Navigation. This is now a most important subject and would be of great use to pilots if it were further amplified. The same applies to No. 7 (Meteorological data) and to No. 8 (Military information and signalling).

No. 9.—Aero Clubs and Societies. It is to be hoped that all the details given here are not so inaccurate as the reference to this Society—the Secretary of which is stated to be Col. Fullerton, and the address, 52, Victoria Street! A more up-to-date knowledge of the work of this Society would have saved the author from this and other blemishes on what would be, if amplified and corrected, a most useful *vade mecum*.

D. S. P. G.

**Th. Scheimpflug's Landvermessung aus der Luft.** By Ingenieur G. Kammerer. (Vienna: 32 pp.) (Reprint from Internationales Archiv für Photogrammetrie.)

THE attempt to create a reliable and speedy method of land surveying from the air should interest sections of our countrymen who appear indifferent to the military significance of aviation.

Scheimpflug set himself the task to obtain maps to scale by means of photographic apparatus. The details of the ingenious instruments that he constructed after thinking out the theoretical solution of the problems involved, the description and discussion of

the processes by which eventually he surmounted many formidable difficulties, are most interesting. It is to be hoped that the technical press devoted to photography or surveying will give their expert attention to the development of Scheimpflug's work. The pioneer and inventor died in August, 1911. Fortunately, Herr Kammerer, who was closely associated with Scheimpflug in much of his work, is devoting himself to the aim of perfecting the inventions.

Scheimpflug, by means of his eight-sided panorama-apparatus and other subsidiary inventions, enables a photo-map to be produced that shows in uniform scale all objects visible from the spot where the view is taken, and not merely a selection of points and lines. His apparatus controls a field of vision fifteen times the circumference of that covered by the largest single objective camera, and the latter, as a rule, would thus require fifteen positions at the same level to cover an equal field. The gain in accuracy by Scheimpflug's method is enormous. Scheimpflug overcame, of course, early in his efforts the difficulty of varying scales in the same photo. Space does not allow one to mention a tenth of the interesting problems tackled by Scheimpflug, but all difficulties have been squarely faced and ingeniously overcome.

Whether or no *minute* accuracy in land surveying from the air is yet attained is perhaps a question for discussion. It is, however, certain that sufficient progress has been made to warrant its economic use. Air surveys for determination of best route for railways in Colonial development, for example, would result in much saving of time and money compared with the present method.

J. E. H.

**The Curtiss Aviation Book.** By Glenn H. Curtiss and Augustus Post. (London: Grant Richards. 1913. Illus. pp. 307. 5s. n.)

One seldom comes across a really entertaining popular book on aviation written by people who can deliver the goods—as they say in America. The Curtiss Aviation Book is this rare thing, and besides providing excellent light reading fills up so many gaps in the history of American aviation that no student can afford to be without it.

The work is divided into seven parts. Part I., by Augustus Post, relates the history of Curtiss up to the time when he turned his attention to aeronautics; in Parts II. and III. Curtiss himself tells—in a charmingly modest and unvarnished way—of his trials and triumphs, from the inauguration of the famous Aerial Experiment Association down to his latest air-boat.

Part IV., on the Real Future of the Aeroplane, is not up to the same high level. The usual predictions are made in the usual way, Capt. Beck, U.S.A., and Lieut. Ellyson, U.S.N., contributing respective chapters on Military and Naval developments, which are dull and dismal.

Part V. is devoted to Practical Flying, and the last Part to a description of the aviators, aeroplane and motor of the Curtiss ilk.

A story about Dr. Graham Bell, as told by Curtiss, though nothing to do with flying, simply clamours for quotation. "Dr. Bell had a strong aversion to the ringing of the telephone bell—the great invention for which he is responsible. I occasionally went into his room and found the bell stuffed with paper, or wound round with towels. 'Little did I think when I invented this thing,' said Dr. Bell one day, when he had been awakened by the jingling of the bell, 'that it would rise up to mock and annoy me.'"

No one can possibly read this book without realising the vast debt flying, and particularly water-flying (or air-boating), owes to Curtiss. He is as immortal as the Wright Brothers.

T. O'B. H.

**The Resistance of the Air and Aviation.** By G. Eiffel. Translated by Jerome C. Hunsaker. (London: Constable & Co. 1913. pp. 242, figs. and plates. 42s. n.)

The value of M. Eiffel's monumental work is already well known, and his results have formed the basis of a great deal of aeroplane design. The pleasure is now added of being able to read the book in English with the good translation that Mr. Hunsaker has made. There are one or two words which might perhaps have been better expressed in English. Thus, in speaking of a plane with a variable angle of incidence, such as the Dunne type, he calls it a "warped wing," which in ordinary aviation parlance suggests the effect obtained with a plane twisted for turning or stabilising. The use of the word "plane" would seem preferable to that of "wing," when used for the supporting surface.

There is an interesting addition to the supplement issued with the original work. This consists of a description of a new wind-tunnel which M. Eiffel has installed so as to obtain higher speed in the tunnel. Instead of using an ordinary Sirocco fan a helicoidal suction blower was installed at the end of a cone-shaped tunnel. The air at this point was, therefore, dealt with at a much lower speed, and could be driven with less expenditure

of power. By this means a uniform airstream 2 metres in diameter was obtained with less than 50 horse power, and a velocity as high as 32 metres per second could be obtained. After a description of the new tunnel there follows an interesting comparison between the results obtained in this new wind-tunnel and those on a full-sized machine. This shows a mean agreement within 1 per cent. Thus M. Eiffel himself effectively disposes of his own contention that the model results should be increased by 10 per cent. to obtain the full size figures, a result which has already been shown to be correct by Mr. Bairstow in his recent paper before the Aëronautical Society.

A further interesting result is the increase in lift and lift-to-drift ratio which is obtained with an increased wind velocity.

The book closes with a brief description of a new apparatus for propeller tests—the torque and thrust of the propeller are measured by an ingenious arrangement of manometer tubes. F. H. P.

### Two Books for Boys

Mr. Herbert Strang is to be congratulated on many good things in "The Air Patrol" (Henry Frowde, Hodder and Stoughton. 6s.). As a yarn for boys it is excellent. There is plenty of fighting, his strategy is good, and his tactics are better. Virtue triumphs over vice, but vice gets a good run for its money. His Pathans, Sikhs, and Kalmucks are quite well drawn, so are his British Officers and his one Ghurka; but the real joy of the book is a priceless Babu clerk "B.A. of Calcutta University," as he informs everyone every time he speaks. His mangled quotations are worthy of Mr. Anstey at his best. I fear, however, that the modern youth, with his intimate knowledge of machines, will not be impressed by Mr. Strang's aeroplane. Mr. Strang seems to lack imagination, for the aeroplane does not appear to possess any features particularly in advance of present-day practice, and is even out-of-date in having a 4-cylinder 100 h.p. motor, yet his two heroes fly it up and down a gorge in the Hindu Khush, where no one but a lunatic would think of flying, and use a ledge on the side of the ravine as a landing place, the rest of the gorge only leaving room to turn at certain wide places some miles apart. A small three-seated dirigible would probably have been better for such jobs, as the Indian Flying Corps will most likely find when they start real work on the North-West Frontier. However, as a story of hairs-breadth escapes the book is fine reading, and it ought to keep many a youngster quiet at those times during the holidays when his elders wish for peace.

"With the Airmen," horrible title (also by Henry Frowde, Hodder and Stoughton. 6s.), is even better calculated to keep a too energetic youth quiet, provided he is young enough, and that he does not get bored at the start with the description—I won't call it an explanation—of how and why an aeroplane flies, for youth hates instruction disguised as entertainment. Incidentally Mr. Grahame-White and his collaborator, Mr. Harry Harper, commit themselves to the statement that "the two-bladed propeller is certainly the most efficient." It is certainly the cheapest, but perhaps the Royal Aircraft Factory can enlighten the authors as to its efficiency. Also the explanation, "the engine does not fire. It is a little sluggish; the cylinders are not yet fully charged with gas," may conceivably be exactly wrong. The cylinders may be choked with too rich a gas. I take it that Mr. Harper is responsible for the literary style, which reminds one of "Little Arthur's England," a method which is quite unnecessary for readers over 12 years old in these days when every boy reads "Treasure Island" at even an earlier age. Still, the adventures of the numerous pilots mentioned, though inaccuracies in detail are frequently obvious to those who remember the incidents, make quite entertaining reading, and the book is likely to be very popular and useful as a present. C. G. G.

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## BACK NUMBERS OF JOURNAL WANTED

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